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CONTRIBUTIONS

TO

PHYSICAL AND MEDICAL KNOWLEDGE,

Principally from the WEST of ENGLAND,

COLLECTED BY

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INTRODUCTION.

In reflecting at various times on the neceſſity and the means of acquiring medical facts, a few ideas have occurred to me ſuch as I have not met with in converſation or in books. I hope they will not be thought miſplaced at the head of a publication like the preſent.

Of human knowledge the moſt important part is that which relates to human nature. He who ſhould undertake to demonſtrate or to enforce this truth would engage in the moſt thankleſs of offices. His arguments and his rhetoric would fall infinitely ſhort of the perſuaſion that already prevails.

The ſcience of human nature is altogether incapable of diviſion into independent branches. Books may profeſs to treat ſeparately of the rules of conduct, of the mental faculties and the perſonal condition. But the moraliſt and the metaphyſician will each to a certain point encroach upon the province of the phyſiologiſt.—Every code of morals muſt ground its precepts on a comprehenſive view of the laws that regulate feeling, and deliver the conditions of an offenſive and defenſive league, having for its object the well-being of individuals. Without accurate ideas therefore of the cauſes that affect the perſonal condition of mankind, how is it poſſible to conceive any progreſs in genuine morality? And will not every addition to this branch of knowledge neceſſarily tend to purify morals—that is, to introduce into the ſocial compact covenants more beneficial to the parties? Without reference to the body, it is equally impoſſible to unfold the nature of the mind. Phyſiology therefore—or more ſtrictly *biology*—by which I mean *the doctrine of the living ſyſtem in all its ſtates,* appears to be the foundation of ethics and pneumatology. If we attend to common converſation, we ſhall find, that few queſtions concerning morals and none at all concerning intellect can be diſcuſſed at any length without a formal appeal to phyſiological axioms.

With this analyſis it would be curious to compare the obſervances of different ages and nations. Few communities, in any degree civilized, are without proviſions for inculcating principles of conduct. In poliſhed ſocieties more than mere preceptive morality is attempted. The profeſſors of liberal education undertake at leaſt to teach the theory of the rational faculties. But here general education ſtops. No ſeminary deſcends to the prime, fundamental, ſure part of ſelf-knowledge. A young perſon may come out into the world ALL ACCOMPLISHED, and not have the ſmalleſt conception of what goes on within his own frame, unleſs he has by chance heard of the circulation of the blood. To their proper interior ſelves preceptor and pupil are alike ſtrangers and alike indifferent. Were it a conſecrated maxim that the faſhion of apparel is more intimately connected with human welfare than the art of preſerving the functions from diſorder, education need not be different from what it now is.—Will it be ſaid that I do the age injuſtice, becauſe the care of the perſon was never carried ſo far as at preſent? But this care, I conceive, is miſnamed *care of the perſon.* To the perſon it is probably oftener injurious than beneficial. It refers to the beholder's eye, not to the feelings of the ſubject on which it is beſtowed. So it can but make the ſurface ſhine, it leaves the ſubſtance below free to rot.

Almoſt every man's ſenſations may convince him what a penalty this order of things impoſes. It is a penalty levied not upon the ſick only, but upon that countleſs multitude who though they reſort to no phyſician, continually carry about with them a comfortleſs conſciouſneſs of exiſtence.

How thoſe with whom it reſts may be induced to open to the riſing generation the moſt beneficial pages in the book of knowledge is a queſtion which I have conſidered with ſome care elſewhere—*(Lecture introductory to a courſe of popular inſtruction on the conſtitution of the human body.* JOHNSON). But among the ſmaller means of rendering education an apprenticeſhip to happineſs and qualifying people to take themſelves into their own keeping, I number the union of *anthropology* with other branches of philoſophy in the ſame publication.

The latter is the proper introduction to the former; it enjoys univerſal currency and may be inſtrumental in procuring for the other the ſame advantage. Accordingly in announcing the preſent collection, I ſaid: "It is not "propoſed to confine the work ſtrictly to medical papers. "With the philoſophy of inanimate nature which bears in "ſo many points upon his art, no practitioner of medicine "ſhould be unacquainted, Nor does any thing ſeem more "demonſtrable than that every liberally educated indivi-"dual ſhould be initiated into the philoſophy of animated "nature. An extenſive ſeries of experiments in the firſt "volume will furniſh a ſtriking example of the connection "between theſe branches of knowledge. And the propriety "of keeping them together ought perhaps on all occaſions "to be held up to contemplation."

My opinion concerning the utility of this aſſociation may be accounted fanciful. Nor will I defend it with much warmth.—The grand expedient for rendering phyſiology popular and medicine certain, is to enlarge our ſtock of obſervations on animal nature. Before the invention of printing, there could be but one practicable way of accompliſhing this purpoſe. Individuals were obliged to commit ſingly to writing what each had ſeen and thought. When there was not matter enough for a proper volume (*justum volumen*), the ideas periſhed with him who had conceived them. Information therefore accumulated ſlowly. In later times a happy expedient preſented itſelf. Periodical publications for preſerving and embodying ſhort and otherwiſe fugitive pieces were ſet on foot. To theſe repoſitories we owe much of the ſuperiority of the modern over antient medicine. To eſtabliſh them on the one hand, and to ſupply them on the other, is perhaps the utmoſt that can be accompliſhed by the co-operation of the members of the profeſſion. But a more effective union of powers is poſſible. If whole communities could but relinquiſh petty intereſts to join in the great work of forwarding the knowledge, and by conſequence diminiſhing the ſufferings, of human nature, the treaſure of phyſiological facts would be eaſily doubled in leſs than twenty years.

As the *manner* in which the labour of ſo many ages may be ſurpaſſed in this ſhort ſpace of time appears to me extremely ſimple, I ſhall preſent a ſketch of it here. Simple as it is however, I am not ſo viſionary as to expect to ſee my method tried. The hiſtory of all uſeful deſigns is nearly ſimilar. Though ridiculed on their firſt propoſal by the ſhallow-minded ſlaves of habit, they are approved at leaſt in principle, and commended, by the intelligent. By degrees the idea becomes generally underſtood and goes on gathering ſuffrages in ſilence. At length, the fullneſs of time arrives. Its execution is propoſed, and mankind are ſurprized to find their opinions united in its favour.

My leading principle is *to provide for the moſt perfect poſſible aſcertainment, and entire publicity of all the phænomena, occurring in charitable eſtabliſhments for the relief of the indigent ſick.* I do not at preſent enter into any enquiry reſpecting theſe eſtabliſhments. On a particular occaſion I endeavoured to throw out ſome ſuggeſtions for extending their benefits without increaſing their expence; and at ſome future period I may reviſe and republiſh my paper, which on its firſt appearance was calculated but for provincial circulation.

The medical functionaries of hoſpitals ſhould be required at fixed, perhaps monthly, periods to furniſh an account of their reſpective departments, particularly noticing ſuch phænomena as ſhould appear to them inſtructive or ſingular.*

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* Since my MS. has been in the printer's hands, I have learned that in the caſe of cancer, ſome ſurgeons at Mancheſter acknowledge the propriety of making the facts occurring in hoſpitals notorious. In a memorial addreſſed to the truſtees of the M. hoſpital, they obſerve, "that if a journal were kept in the manner they propoſe, *and to be open to general inſpection*, much good would ariſe to the objects of theſe inſtitutions, and general benefit to the community. We have therefore agreed to keep an exact account of each caſe of cancer which ſhall come under our care, in which ſhall be recorded a faithful hiſtory of the diſeaſe, &c. &c. &c." The propoſal was adopted. (*Simmons on the Caeſarean operation* 1799. *Vernor and Hood.* p p. 74—5.). It is, I preſume, impoſſible for the human imagination to ſupply any ſpecious reaſon for the above limitation of the principle. As medicine fails in ſo many caſes beſides cancer, there is juſt the ſame reaſon for "recording a faithful hiſtory of the diſeaſe, with its "attendant circumſtances; the effects of medicines; and of operations "when neceſſary; together with all the collateral helps to be gained "by an enquiry into conſtitutional habits and diſeaſes" There is alſo in other inſtances juſt the ſame reaſon for giving publicity to the information which all this care ſhall have accumulated, in order that as many minds as poſſible may be ſet at work to detect the means of eaſe and health. In the part of the above quotation printed in italics, I am aware that *general* may mean *confined to the hoſpital ſurgeons.* When the rules of hoſpitals are framed according to the ſyſtem, moſt conducive to the good of the patients they receive, and of the benefactors who uphold them, the word will be employed in its uſual unlimited ſignification.

To theſe meetings all the practitioners of the place and neighbourhood, together with ſubſcribers to the charity, ſhould have free acceſs. When the ſtatement contained any thing uncommonly intereſting, a commiſſioner or committee of verification ſhould be appointed to examine the circumſtances. In caſes not admitting of delay, the attending phyſician or ſurgeon ſhould call in one or more commiſſioners during the intervals of the ſittings. It is of courſe that the facts thus acquired ſhould be ſubject to the remarks of the parties preſent, and that the more ſelect ſhould be given to the public in ſome commodious form.

In large and capital cities, the eſtabliſhments that are ſupported by general contribution might thus be rendered moſt effectually ſubſervient to the general good. There, the tranſactions of many hoſpitals might be brought under review at once, and contraſted. There alſo, will be the greater chance that particulars in the reports ſhall ſuggeſt diſcoveries to ſome one among a numerous audience, though the very ſame particulars, for want of neceſſary previous aſſociations, ſhould be utterly loſt upon the reporters themſelves; juſt as ſparks fail to kindle a blaze when they fall upon incombuſtible materials. So intimate likewiſe has ſcience rendered the connection between the organic and inorganic kingdoms of nature, that the unprofeſſional philoſopher, well apprized of the deſiderata of medicine, may be ſometimes more in a capacity to ſupply them, than the unphiloſophical practitioner.

After conſidering the ſtake which ſociety has in medicine; how often in a man's life it may, according to its power and adminiſtration, wound or ſoothe his perſonal feelings and his ſympathies; let him imagine himſelf preſent at one of theſe ſittings where the buſineſs is carried on with a ſpirit adequate to its importance. What motives for ſelf-congratulation, and for congratulating his whole ſpecies, will the ſcene before him offer! the art of moſt immediate and moſt univerſal concern, brought out of that darkneſs in which none can diſtinguiſh whom it preſerves and whom it deſtroys! its doubts ſolved! its contradictions reconciled! the cauſes of phænomena, where eaſe and exiſtence are at iſſue, detected! light and order ſuddenly ſpread through trains of ideas that had long been vainly ſtruggling in the minds of the ingenious! the ſtores of the moſt knowing augmented! the powers of the moſt acute ſharpened! the intereſt of all claſſes promoted! the fortunate ſon of Æſculapius retiring with better informed judgment to the manſions of the opulent! the humbler practitioner carrying away comfort to the peaſant and the pauper! All things conſidered, I know not if thoſe aſſemblies which fix the deſtiny of diſtant nations and unborn generations would appear in the eye of humanity more auguſt, or be found more capable of inſpiring exalted ſentiments.

Europe abounds in private medical ſocieties. It abounds in public philoſophical ſocieties, purſuing with ſucceſs every ſpecies of knowledge, but the knowledge of man. Theſe are the disjoined elements of the aſſociation here ſuggeſted. They do not require a great deal more than to be united. The leiſure of the extra-profeſſional members, their ingenuity, and (what in phyſiological reſearches is often equally important) their wealth would enable them to purſue many trains of experiment to which the means of the profeſſional members would be in every way unequal. The ſchool of Haller, (from whoſe reſearches, together with thoſe of his pupils, modern celebrated doctrines derive their principles,) ſhews how much may be accompliſhed in a ſhort time, when there is any perſon or occaſion to inſpire energy. Many ſimilar examples would have occurred in the hiſtory of our art, if all who have profeſſed teaching had practiſed thinking. But, according to my ſyſtem, it will be nearly indifferent to the progreſs of phyſiology, by whom lecturers' chairs, or medical offices, are filled. All the combinations of which the ſtate of knowledge is at any time capable, would be formed by genius, and proſecuted by induſtry.

Many would undoubtedly ſhrink from ſuch an effort of attention and ſuch a teſt of ability as I require.* To be phyſician to an infirmary is often more a title than a function, and retained out of jealouſy or pride. Let it not however be forgotten that I ſuppoſe my deſign impracticable till a diſcovery in phyſiology ſhall be capable of exciting as warm ſenſations as a miniſterial harangue. In ſuch a ſtate of national feeling nothing will appear a labour of ſupererogation. As medical philoſophy gains more of the public attention, medical practitioners will become more and more the devoted ſervants of their art. Such as is the community, ſuch will ever be medicine. The more intelligence in one, the leſs quackery, the leſs intrigue, the leſs helpleſsneſs in the other. Public participation would rouſe the ingenious from their indolence, and deprive the incapable of that credit which the public ignorance ſuffers them to acquire, and which they abuſe "*to the grievous hurt, damage, and deſtruction, of many of the king's liege people.*" It is often the conſciouſneſs of the part mankind take in their exertions, that ſtimulates the ſoldier in the field, and the orator in the council. Under the ſame powerful motive the phyſician would alſo perform wonders. But no ſuch motive has ever operated upon medicine. The author once inſcribed a book to the diſcoverer of a remedy which has reſcued numbers from the tortures of the ſtone, as being in his opinion the member of ſociety moſt deſerving honor. The name, from a feeling that it ought long before to have been in every man's mouth, he omitted. Not leſs perhaps than a hundred well-informed perſons have ſince aſked him whom he meant. So true is it that few (according to the language of the old ſtatute) "*diſcern the uncunning from the cunning*" in our art; and that in the great world, where celebrity and profit are chiefly to be acquired, there is much leſs chance of acquiring them by the moſt tranſcendant merit, than by a winning or impoſing carriage.

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* In Germany there are many ſmall hoſpitals and diſpenſaries of recent date. The perſons who preſide over them have almoſt univerſally conſidered it as a duty to publiſh this part of their practice, that the benefit might extend beyond the immediate objects of the charities. I have beſide me half a dozen ſuch publications and ſuppoſe I could name twenty more. It appears that the continental phyſicians are emancipating themſelves from the ſhackles of old medical theories, that they are fixing their undivided attention upon the courſe of nature, and that for them a new and auſpicious æra in medicine is beginning.

By the indolent and the ill-informed I ſhall perhaps be told that our hoſpitals are already in able hands and that every thing uſeful to the preſent age and to poſterity will be faithfully recorded: Conſequently that no good can accrue from the cumberſome regulations I propoſe. But I, who could name a variety of hoſpitals, which in a long courſe of years have furniſhed nothing or next to nothing to medical philoſophy, muſt be ſlow to believe in this univerſal vigilance. To many of our predeceſſors and many of our contemporaries the healing art is indeed deeply indebted for what they have recorded of their hoſpital practice. Nor would I deſire a more concluſive argument. For if a part is ſo valuable, what would the whole be? And what, according to a juſt eſtimation, are the facts that have been preſerved, but fragments of a mighty wreck, demonſtrating the value of the maſs that has periſhed?

The profeſſion and the public being ſo far excited, the reſt would eaſily follow. No remarkable phænomenon in private practice would remain unexamined and unauthenticated. Curioſity would have every where free ſcope: exerciſe would immenſely improve the talent for obſervation; and no great improvement in the talent for obſervation can take place without a proportionate increaſe of ſucceſs in practice.

Some readers may find it difficult to believe either that there is ſuch a deficiency of phyſiological facts as I inſinuate, or that an addition to the number would be cheaply purchaſed by any efforts and almoſt any ſacrifice. But theſe ſceptics ſhould be reminded that ſcience improves juſt as facts multiply. The greater the number of phænomena known, the better is each underſtood. It will hardly be denied that books of medicine abound in falſified or erroneous or mixed narrations much more than in genuine facts. Their perplexed and abſurd contents in general excite repugnance. By whom are they peruſed but by ſuch as hope of profit or ſuffering impels to turn over a mountain of rubbiſh for the chance of picking up a grain of gold?—But ſuch during its infancy is the chaotic ſtate of the records of every ſcience. Something is perceived, much imagined and little underſtood. Authors blend what they imagine with what they perceive; nor have they the prudence to refrain from attempting to explain what they do not underſtand. But in time they acquire patience to look more ſteadily and learn to diſtinguiſh better. Then the miſts of imagination gradually diſappear and objects are beheld in their natural ſhape and order. Thus exact information is procured; and it is among the precious advantages of exact information that, in adulterated narratives, it enables us to ſeparate the falſe from the true, and prevents us from being deceived by wrong teſtimony. It would be impoſſible to impoſe a ſeries of forged obſervations on the aſtronomers and chemiſts of Europe. And ſuch as the ſpurious half-alchemical chemiſtry of the two laſt centuries is in our eye, ſuch will our medicine appear when equal zeal ſhall have been kindled for phyſiological reſearches.

Nor is it neceſſary to appeal to the general analogy of ſcience, to demonſtrate our deplorable deficiency in phyſiological facts. It is impoſſible to think, or converſe, or write concerning animal nature, without being ſtopped for want of facts. Our lateſt ſyſtematic writer remarks that "as a great number of unconnected facts are difficult to "be acquired, and to be reaſoned from, the art of medicine "is leſs efficacious under the direction of its wiſeſt prac-"titioners." Yet it is certain that to obſerve well and to connect well the phænomena occurring in a ſingle caſe only, may be followed by the effect of ſaving many lives.

Another idea which I conſider as of ſome importance long ſince preſented itſelf to me. It has had a local circulation: and on the preſent occaſion I ſhall lay it before the public at large. It bears ſome analogy to the preceding, inaſmuch as it would render hoſpitals a greater ſource of improvement to medicine. Both ideas ſhould be moulded into one ſyſtem. But it is poſſible to realize one without the other; and I expect that if either is realized, it will be the latter.

My ſecond propoſal is *that the phyſicians and ſurgeons of hoſpitals be changed, or partly changed, every year, or every ſecond year: that, if the average number of reputable phyſicians and ſurgeons reſiding in or near a place be ſufficient for two changes, thoſe who go out firſt be not re-elected till at leaſt two periods ſhall have elapſed; and that the excluſion be prolonged according to the probability of a proper ſucceſſion.*

I ſhall endeavour to ſhew that this plan is practicable, that it promiſes ſuperior advantages in more than one reſpect, and that elections for an indefinite term are unjuſt towards the majority of the profeſſion, and comparatively prejudicial to the public.

The rotation-ſcheme is practicable, for it is practiced, at leaſt without inconvenience, in more than one large city within the Britiſh dominions. This matter of fact will be proved, in the inſtance to which it refers, by the following letter from a phyſician, whoſe talents and reſpectability are ſufficiently known. An extract would have been ſufficient for my immediate purpoſe. But, as the letter deſcribes certain humane arrangements that might be introduced elſewhere and is throughout intereſting, I cannot prevail on myſelf to give it in a mutilated form.

Letter from R. CLEGHORN, M. D. *Profeſſor of Medicine in the Univerſity of Glaſgow, to T. Beddoes, M. D.*

Glasgow, July 15. 1797.

SIR,

It gives me great pleaſure to hear that our infirmary has been honoured with the approbation of ſuch a judge as Mr. WATT,* and I ſhall moſt willingly give you every information in my power concerning it.

Our printed papers are not worth ſending. They refer merely to the admiſſion of patients, the rights annexed to different ſubſcriptions, and other things of that ſort: however, if you ſhall ſtill deſire to ſee them, I will forward ſome copies by the firſt private opportunity.

Our infirmary owes much to its ſituation, which is high and open all around, with an elegant court before, and a large convenient garden behind. From a neighbouring canal, water riſes between 30 and 40 feet above the foundation, and by forcing pumps, it is raiſed to the higheſt wards, each of which being ſupplied with a water cloſet, we are ſeldom troubled with ſuch ſmells as you may remember on the ſide ſtairs of the Edinburgh infirmary. Our wards too are more convenient. There are no projections from the walls, and the bed-poſts are all of iron. In ſhort, every part is conſtructed, as much as poſſible, according to a report made to the late king of France, by a Committee of the academy, who were deſired to point out the faults of the Pariſian hoſpitals, and to ſpecify the beſt mode of correcting them. Happening to get their report and plans, I gave them to Mr. Adams, who ſtudied them carefully, and with much profit.

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* I had mentioned my idea to Mr. W. who told me that my theory had been anticipated in practice, and referred me to Dr. Cleghorn.

The management of our infirmary is veſted in twenty-five members, of whom fifteen are official, (ſuch as the member of parliament for the city, the provoſt (anglicé mayor) of Glaſgow, the preſident of the faculty of phyſicians and ſurgeons, &c.) the other ten are elected annually from among the ſubſcribers. The managers have ſtated quarterly meetings, at which they appoint committees, and examine every tranſaction connected with the buſineſs of the infirmary, and the reſult of all their proceedings, together with an account of the money received and expended, of the patients admitted and diſmiſſed, &c. is laid every year before a general court of contributors, and afterwards publiſhed.

A committee of managers meet every friday in the infirmary, to examine the liſt of patients admitted or diſmiſſed, and to take cognizance of any other material occurrences. Beſides the managers, each of whom viſits in his turn, twelve ſubſcribers, not in the management, are named every year, as viſitors for that year. The viſitor goes daily, but at no ſtated hour, through every part of the houſe; he taſtes the food, he examines the nurſe reſpecting the behaviour of the patients, and the patients reſpecting the conduct of the nurſe in the preſence of each other. He remarks daily his report in a book kept on purpoſe, and each report is carefully inveſtigated by the weekly committee who check the firſt appearance of oppreſſion among the nurſes, or of irregularity among the patients. This plan keeps all the ſervants on the alert from morning to night, and it is unſpeakably gratifying to the poor patients to ſee ſo many intereſted in their welfare.

Two phyſicians are elected annually. Hitherto they have attended 3 *months alternately; next winter they may probably attend both together for the convenience of the clinical ſtudents who have only an hour for the infirmary.*

Four ſurgeons are elected annually. Two go out every year, and thoſe who have attended two years, are not again eligible till after two years. The ſurgeons regulate their attendance by their own convenience; hitherto each has generally attended 3 *months in turn.*

A phyſician and ſurgeon attend every day at ten o'clock. Each examines his own patient, dictates reports, &c. juſt as in Edinburgh. They then viſit the antichamber which is on the firſt floor, and there, beſides examining the patients recommended for admiſſion, they give advice to many others, ſometimes thirty in a day. The attending phyſician and ſurgeon are officially members of the weekly committee, which they regularly attend along with their clerks to give information, eſpecially concerning thoſe patients who have been in the houſe above two months. If there be a proſpect of relief, the patients are allowed to remain; if not, the recommender is obliged to remove them.

Two clerks live in the houſe. One is apothecary and ſurgeon's clerk, the other is phyſician's clerk. They write the caſes and reports, viſit the patients in the evening, &c. as in Edinburgh. They have bed, board, and waſhing, but no ſalary. No medical attendant has any ſalary.

Before any operation, both phyſicians, and all the ſurgeons are called to conſultation. After examining the patient, and hearing the caſe, each gives his opinion (no ſtudent except the clerks being preſent) and the buſineſs is regulated by the opinion of a majority. We have a very convenient theatre in which the ſtudents ſee the operations very diſtinctly.

Our patients once amounted to ninety-four, which is the greateſt number we ever had; laſt week they were ſeventy-three. When the houſe ſhall be completely furniſhed, it will be able to contain one hundred and forty-five, with their neceſſary attendants.

Theſe are the chief things that occur to me. If you wiſh for any further information it will give me the utmoſt pleaſure to communicate it. With great reſpect, I am, Sir, your obedient ſervant,

ROBERT CLEGHORNE.

In the *clinical* wards of the Edinburgh infirmary, where accuracy is more eſpecially neceſſary, ſince minute reports of the condition of all the patients are taken down each day, the phyſicians are changed twice in ſix months. And no eſſentially inherent inconvenience has ever been felt or imagined, in conſequence of the transfer of the patients at the end only of a quarter of a year.

The caſe of Mancheſter, where I believe an entire change of the ordinary phyſicians and ſurgeons not long ſince took place, appears to me ſtrictly in point. A regular ſucceſſion has not, as far as I know, been eſtabliſhed there. But on the reſignation of the old ſet of practitioners, no diſadvantage to the ſick was complained of. Nor is it unlikely that this event, by awakening the minds of profeſſional men and of others, contributed in ſome meaſure to the execution of a moſt ſucceſsful plan for preventing the diſſemination of febrile contagion, and generally promoted the purpoſes of the charity.

From the aſcertained motives of human action we may calculate that excluſive poſſeſſion during life or pleaſure, will have in part the baneful effects of monopoly. And that a perpetual quick ſucceſſion of profeſſional attendants will operate as a conſtant ſtimulus to exertion.

I am fully aware how liable ſuch a remark is to invidious interpretation. I may be aſked whether I mean to inſinuate a general charge of negligence. But I am not ſpeaking of culpable remiſſneſs. There is a wide difference between decent attendance, and that ardour which ſprings from emulation and the enjoyment of a freſh occupation conjointly. It is the latter one would wiſh always to ſecure, when it can be had.

Having ſometimes enquired, I have almoſt as conſtantly been informed, that *cæteris paribus*, the period immediately following their election, is that during which phyſicians and ſurgeons have moſt zealouſly intereſted themſelves in the concerns of an hoſpital. It may indeed be expected that phyſicians and ſurgeons, in earneſt about their profeſſion and not yet fully employed, will regularly devote a large portion of time to hoſpital duty, whereas the fully employed can only make curſory and occaſional viſits. Is it not then to be expected, that a rotation can on no principle diminiſh the quantity of care beſtowed upon the ſick, and that it will probably on the whole much increaſe it?

In cities, and in the country, a conſiderable number of perſons above the neceſſity of charitable aid, entruſt their health to practitioners not belonging to any infirmary. Of this number it is the intereſt to have the advantages of medical information more equally diffuſed. And unvaried poſſeſſion, when it does not abſolutely cloy or diſguſt, is ſo well known to lower the tone of feeling, and impair the energy of exertion; that under a ſenſe of rivalſhip occaſional ſuperintendance may be an equal, or a greater ſource of improvement.

The wrong done to the body of excluded practitioners (often men of the firſt talents in their reſpective profeſſions) is much more flagrant than the diſadvantage to the public, or to the charity. This is indeed a ſpecies of injury that ought never to be mentioned but in terms of the moſt pointed reprobation, were it not certain that no ſyſtem, capable of reconciling the intereſts of all parties, has generally been apprehended to be practicable.

When one aſks—*why are votes ordinarily given for or againſt ſuch a candidate?* there is danger of appearing perſonal. But certainly ſeldom in conſequence of impartial *compariſon* grounded on pure reſpect to merit, and the intereſts of the houſe. Chance has full dominion here. A man is born to ſuch or ſuch a connection. He is thrown among this or that ſet. So he gets his majority or minority. Reverſe but this one circumſtance, and the reſult of the election will generally be reverſed. Few voters are capable of diſcriminating between a Sydenham, and a competitor whom no opportunities could ever enable to add an iota to the ſcience of human nature. And ſome voters, though aware of the difference, would not have magnanimity to prefer the eternal benefactor of his ſpecies, to a countryman, a couſin, or a clubmate. Where therefore the votes ſtand as 400 to 40, the qualifications may very well be in the inverſe proportion of the numbers. So perpetually do the cauſes of vogue in medicine remind the diſcerning ſpectator of Pope's queſtion concerning another ſort of favour!——

> What ſhook the pit, and made the people ſtare?

It would therefore be idle to object that the circulating ſyſtem may place the ſick in inferiour hands. The ſyſtem of perpetuity (for what any unprofeſſional perſon can tell) may *keep* them in inferiour hands. Nor is it meant that every unowned vagabond, ſtyling himſelf doctor or ſurgeon, ſhall be reputed to have an indefeaſible title to an appointment at an infirmary. Subject the candidate to a reaſonable probation; and then honeſtly allow fair preſumptive claims, beyond which the judgment of a body of ſubſcribers can never penetrate. Such claims are conſtituted by public ſervices in the cauſe of a man's profeſſion, by an adequate education, by a ſhare of experience, by good ſenſe and good conduct during a term of reſidence, long enough to afford ſome inſight into character.

No equitable reader will conſtrue an aſſertion of the juſt expectations of one ſet of men into diſparagement of another ſet. It is only contended that if the office of public phyſician or ſurgeon be a burden, it ought to be equally borne—if an honour, it ought to be fairly ſhared. If there be practitioners out of infirmaries of as extenſive reputation, as ardent in the cultivation of their departments, and to whom perſons as high in rank, as dear to themſelves, and as valuable to the world, entruſt themſelves, they are ſurely to be preſumed not leſs qualified to take charge of the indigent ſick.

There is reaſon for ſuppoſing that the ſuggeſted change would procure an occaſional augmentation of the funds—an advantage not to be deſpiſed at a time when in ſome places diſtreſs has neceſſitated extraordinary meaſures, and obvious cauſes threaten defalcation of revenue. Individuals acquainted with medical men of unexceptionable character, of excellent general abilities, and unwearied attention to their profeſſion, but leſs fortunate than ſome of their brethren, would be ſtrongly induced to contribute to an inſtitution, of which *their* ſavourites may in turn hope to participate in the advantages, and to promote the purpoſes. The ſame motive may render the profeſſion at large, more earneſt in the exertion of their influence to procure ſubſcriptions.

Were this propoſal to engage the public attention, objections of detail would be urged by the intereſted, and perhaps by others. But ſuch oppoſition every uſeful principle muſt expect to encounter. As many of the London hoſpitals have been converted into little ſchools of medicine, it may be ſaid, that a reciprocation of phyſicians and ſurgeons would diſturb the order of inſtruction. The inconvenience might be obviated, by allowing perſons to lecture during the ſuſpenſion of their other office. With this, moſt of the courſes have no inſeparable connection; and whatever is the preſent motive of theſe exertions, whether hope of profit or fame, it would remain. I rather think, however, that great good would ariſe, in different ways, from a ſucceſſion of lecturers. The parties in expectation would take care to inſtruct themſelves in many things, of which they are now content to remain ignorant or half-informed. By the efforts of various minds thus excited, many more combinations would be formed, than if ſuch an incentive did not exiſt; and by theſe the public would inevitably profit, as well as the audience. That the talents of many great men are unproductive, for want of more diffuſed encouragement and opportunities, will be evident to thoſe who are not weak enough to take it for granted that the moſt ſucceſsful are *ipſo facto* the moſt meritorious. To ſuppoſe that phyſicians who happen to be beſt known to a body of electors, are preciſely the men beſt qualified for an important medical ſtation, derives from that credulity upon which was built the dogma of papal infallibility; and which in the preſent age, is more apparent in the ſentiments of mankind reſpecting medicine than in thoſe reſpecting religion. Had room been left for ingenious men to emerge, we might have ſeen during the ſame period, a dozen inſtead of a ſingle Pott, and a ſingle Hunter..

I doubt not but ſeveral of my readers have anticipated theſe arguments, and could furniſh additional ones. To thoſe of ſlower apprehenſion, I hope I have proved (or enabled them to prove to themſelves) that a more liberal ſyſtem would better ſecure both the primary and ſecondary ends of an infirmary: that by being frequently relieved each man would be more alert at his poſt; that no other plan can be deemed equitable towards the faculty at large; that ſo juſt a regulation while it quickened the diligence of ſome, would enlarge the experience of others: that, under its influence, theſe charities might be expected to furniſh ampler ſupplies than they have yet done to the maſs of medical knowledge: that by conceding equally to opinions, of which none can have any ſolid title to preference, it might bring more contributions to the general fund, exempt candidates from the humiliating preliminary of a canvaſs, mitigate electioneering animoſities, and diſentangle CHARITY from CABAL.

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* As Dr. Gibbes has related an experiment of his own on the Sodbury ſulphate of ſtrontian in *Mr. Nicholſon's journal*, *March* 1799, without referring to any other perſon's previous obſervations, it ſeems but juſt to mention the following circumſtances. In the collection of the Rev. Mr. Richardſon at Bath, a friend of mine (Mr. Notcutt) inſtructed by Mr. W. Clayfield's ſpecimens, pointed out ſome ſulfate of ſtrontian. Mr. Richardſon gave a piece to Dr. Gibbes in January laſt. I had exhibited this ſubſtance, as found in other places near Briſtol, to a large audience nine months before, viz. in ſpring 1798, and had ſent ſpecimens to Mr. W. Henry, who communicated the fact to the philoſophical ſociety at Mancheſter. Dr. Gibbes does not, undoubtedly, mean to claim the diſcovery. He knew laſt year that Mr. Clayfield was analyſing the foſſil. Hundreds of perſons might have anticipated Mr. C. in announcing the fact. It is, indeed, I obſerve diſtinctly noticed in the *appendix to the monthly review*, vol. xxv. p. 580. June, 1798. The manner, in which the ſulfate was detected in this neighbourhood, is exactly related below. Editor.

To Dr. BEDDOES,

AND

TO THE SUBSCRIBERS TO THE

PNEUMATIC INSTITUTION,

THESE ESSAYS ARE RESPESTFULLY

INSCRIBED.

AN ESSAY

ON

HEAT, LIGHT, and the Combinations of LIGHT.

A deſire of improvement, and a peculiar ſpirit of philoſophical inveſtigation, productive of the greateſt diſcoveries in the moſt important ſciences, have eminently characteriſed this century.

Philoſophers, not contented with examining the aſſociations of thoſe complex perceptions, to which the metaphyſicians have given the name of abſtract ideas, have obſerved nature, diſcovered effects, and erected their theories upon trains of connected ſenſations, called facts.

From hence ariſes the ſuperiority of preſent philoſophical ſyſtems; though theſe ſyſtems are far from that perfection, which they appear capable of attaining by the new mode of inveſtigation.

The ſciences have not been equally improved; thoſe dependent for their exiſtence on experiment, and the obſervation of phyſical phænomena, though newly diſcovered, have made the moſt rapid advances towards perfection.

Chemiſtry, which aroſe from the ruins of alchemy, to be bound in the fetters of phlogiſton, has been liberated, and adorned with a beautiful philoſophic theory. The numerous diſcoveries of Prieſtley, Black, Lavoiſier, and the other European philoſophers in this branch of ſcience, afford ſplendid proofs of the increaſing energies of the human mind.

From the application of Chemiſtry to the diſcovery of the laws of organic exiſtence, mankind had hoped to derive the greateſt advantages; from this ſource they expected the perfection of phyſiological ſcience; but their hopes have been in a great meaſure fruſtrated; and if we except the theories of a celebrated medical philoſopher, Dr. Beddoes, it will be found that chemiſtry has as yet afforded but little aſſiſtance in the cure of diſeaſes, or in the explanation of the laws of organic exiſtence.

Our ignorance of the compoſition of organic matter, and of the changes effected in the blood by oxygen gas, is a conſiderable ſource of the imperfection of medicine. A more intimate acquaintance with thoſe important parts of chemiſtry would tend, not only to the elucidation of that branch of philoſophy, but to the production of the moſt beneficial effects in medicine and phyſiology, ſciences of the utmoſt importance to man; ſciences, from the perfection of which he may hope to eradicate a great portion of the phyſical and moral evil to which he is ſubject.

LIGHT has been heretofore little conſidered in chemical theory; its affinities have never been inveſtigated. A ſubſtance of the greateſt importance to organic exiſtence has been very little regarded, but in a phyſical view, as a ſtimulus, and as the ſource of the moſt numerous and pleaſurable of our ſenſations.

The planetary motions, thoſe wonderful phænomena, and the laws by which they are governed, appear to be deſigned for the expreſs purpoſe of ſupplying the whole of the ſolar ſyſtem with a certain neceſſary quantity of light.

The general analogy of nature, the wonderful ſimplicity of cauſes and complexity of effects, would alone tend to prove that this ſubſtance is ſubſervient to other purpoſes than thoſe of viſion and vegetation. Since light and heat are uſually concomitant, ſince there is rarely a conſiderable degree of one without the other, philoſophers have queſtioned whether they are not cauſe and effect; and M. Lavoiſier is one of theſe philoſophers. He ſays:* "La lumiere, eſt elle une modification du calorique, ou bien le calorique, eſt il une modification de la lumiere?" I have made an experiment which ſeems to demonſtrate directly that light is not a modification, or an effect of heat.

EXPERIMENT I.

A ſmall gunlock was procured, armed with an excellent flint. This lock was elevated by means of two iron ſprings on the ſtand of the receiver of an air-pump. A ſlight iron wire was affixed to the trigger, brought through a hole made in the centre of the ſtand, and cemented into the hole with wax, ſo as to exclude entirely atmoſpheric air from the receiver. The receiver was exhauſted, and the lock ſnapped; but no light was produced. The receiver was filled with carbonic acid, and the lock again ſnapped, with the ſame reſult; no light was produced. Small particles were ſeparated from the ſteel, which on microſcopic examination evidently appeared to have undergone fuſion.

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* Traité elementaire. t. 1. p. 6.

If light was a modification, or an effect of heat, it muſt have been produced in this experiment; ſince the heat generated by colliſion was ſufficient to fuſe ſteel, a degree of heat much above that improperly called a white heat.

Light then cannot be caloric in a ſtate of projection.

Nor can it be, as ſome philoſophers ſuppoſe, a vibration of the imaginary fluid ether. For even granting the exiſtence of this fluid, it muſt be preſent in the exhauſted receiver, and in carbonic acid gas, as well as in atmoſpheric air; and if light is a vibration of this fluid, generated by colliſion between flint and ſteel in atmoſpheric air, it ſhould likewiſe be produced in the exhauſted receiver, where a greater quantity of ether is preſent, which is not the caſe.

Since light is neither an effect of caloric, nor of an etherial fluid ſuppoſed to be extended through ſpace,* and as the impulſe of a material body on the organ of viſion is eſſential to the generation of a ſenſation, light is conſequently matter of a peculiar kind, capable when moving through ſpace with the greateſt velocity, of becoming the ſource of a numerous claſs of our ſenſations.

Matter is poſſeſſed of the power of attraction. By this power the particles of bodies tend to approximate, and to exiſt in a ſtate of contiguity. The particles of all bodies with which we are acquainted, can be made to approach nearer to each other, by peculiar means, that is, the ſpecific gravity of all bodies can be increaſed by diminiſhing their temperatures. Conſequently (on the ſuppoſition of the impenetrability of matter) the particles of bodies are not in actual contact. There muſt then act on the corpuſcles of bodies ſome other power, which prevents their actual contact; this may be called repulſion. The phænomena of repulſion have been ſuppoſed, by the greater part of chemical philoſophers, to depend on a peculiar elaſtic fluid; to which the names of latent heat, and caloric, have been given. The peculiar modes of exiſtence of bodies, ſolidity, fluidity, and gazity, depend (according to the caloriſts) on the quantity of the fluid of heat entering into their compoſition; this ſubſtance inſinuating itſelf between their corpuſcles, ſeparating them from each other, and preventing their actual contact, is, by them, ſuppoſed to be the cauſe of repulſion.

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* The philoſophers who ſupport this opinion ſuppoſe the univerſe a plenum; amongſt theſe is the great Euler. It may not be amiſs to obſerve on this ſubject, that to ſuppoſe the univerſe a plenum, and conſtituted a plenum by an elaſtic fluid, is abſurd. It is eſſential to the elaſticity of a fluid, that it be capable of compreſſion; that is, of filling leſs ſpace than it before exiſted in. Now as different bodies cannot exiſt at the ſame time in the ſame place, there muſt neceſſarily be a void ſpace between the particles of an elaſtic fluid, ſuch as ether is aſſumed to be, which overturns their hypotheſis.

Other philoſophers, diſſatisfied with the evidences produced in favour of the exiſtence of this fluid, and perceiving the generation of heat by friction and percuſſion, have ſuppoſed it to be motion.*

Conſidering the diſcovery of the true cauſe of the repulſive power as highly important to philoſophy, I have endeavoured to inveſtigate this part of chemical ſcience by experiments: from theſe experiments (of which I am now about to give a detail,) I conclude that heat, or the power of repulſion, is not matter.

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* It was foreign to the deſign of this eſſay to give the hiſtory of the opinions of the different philoſophers on the cauſe of heat, as well as an account of the different experiments that have heretofore been made on it. The ſhort view of the phænomena of repulſion given after the experiments on the cauſe of heat, is deduced from the experiments of Black, Crawford and others, experiments which are too well known by chemical philoſophers to need quotation.

The phænomena of repulſion are not dependant on a peculiar elaſtic fluid for their exiſtence, or Caloric does not exist.

Without conſidering the effects of the repulſive power on bodies, or endeavouring to prove from theſe effects that it is motion, I ſhall attempt to demonſtrate by experiments that it is not matter; and in doing this, I ſhall uſe the method called by mathematicians, reductio ad abſurdum.

Let heat be conſidered as matter, and let it be granted that the temperature of bodies cannot be increaſed, unleſs their capacities are diminiſhed from ſome cauſe, or heat added to them from ſome bodies in contact.

Now the temperatures of bodies are uniformly raiſed by friction and percuſſion. And ſince an increaſe of temperature is conſequent on friction and percuſſion, it muſt conſequently be generated in one of theſe modes.

Firſt, either from a diminution of the capacities of the acting bodies from ſome change induced in them by friction, a change producing in them an increaſe of temperature.

Secondly, or from heat communicated, from the decompoſition of the oxygen gas in contact by one or both of the bodies, and then friction muſt effect ſome change in them (ſimilar to an increaſe of temperature) enabling them to decompoſe oxygen gas; and they muſt be found after friction, partially or wholly oxydated.

Thirdly, or from a communication of caloric from the bodies in contact, produced by a change induced by friction in the acting bodies, enabling them to attract caloric from the ſurrounding bodies.

Now firſt let the increaſe of temperature produced by friction and percuſſion be ſuppoſed to ariſe from a diminution of the capacities of the acting bodies. In this caſe it is evident ſome change muſt be induced in the bodies by the action, which leſſens their capacities and increaſes their temperatures.

EXPERIMENT II.

I procured two parallelopipedons of ice,* of the temperature of 29°, ſix inches long, two wide, and two thirds of an inch thick: they were faſtened by wires to two bars of iron. By a peculiar mechaniſm, their ſurfaces were placed in contact, and kept in a continued and violent friction for ſome minutes. They were almoſt entirely converted into water, which water was collected, and its temperature aſcertained to be 35°, after remaining in an atmoſphere of a lower temperature for ſome minutes. The fuſion took place only at the plane of contact of the two pieces of ice, and no bodies were in friction but ice. From this experiment it is evident that ice by friction is converted into water, and according to the ſuppoſition its capacity is diminiſhed; but it is a well known fact, that the capacity of water for heat is much greater than that of ice; and ice muſt have an abſolute quantity of heat added to it, before it can be converted into water. Friction conſequently does not diminiſh the capacities of bodies for heat.

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* The reſult of the experiment is the ſame, if wax, tallow, reſin, or any ſubſtance fuſible at a low temperature be uſed, even iron may be fuſed by colliſion, as is evident from the firſt experiment.

From this experiment it is likewiſe evident, that the increaſe of temperature conſequent on friction cannot ariſe from the decompoſition of the oxygen gas in contact, for ice has no attraction for oxygen. Since the increaſe of temperature conſequent on friction cannot ariſe from the diminution of capacity, or oxydation of the acting bodies, the only remaining ſuppoſition is, that it ariſes from an abſolute quantity of heat added to them, which heat muſt be attracted from the bodies in contact. Then friction muſt induce ſome change in bodies, enabling them to attract heat from the bodies in contact.

EXPERIMENT III.

I procured a piece of clock-work ſo conſtructed as to be ſet to work in the exhauſted receiver; one of the external wheels of this machine came in contact with a thin metallic plate. A conſiderable degree of ſenſible heat was produced by friction between the wheel and plate when the machine worked uninſulated from bodies capable of communicating heat. I next procured a ſmall piece of ice;* round the ſuperior edge of this a ſmall canal was made and filled with water. The machine was placed on the ice, but not in contact with the water. Thus diſpoſed the whole was placed under the receiver (which had been previously filled with carbonic acid), a quantity of potaſh (i. é. cauſtic vegetable alkali) being at the ſame time introduced.

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* The temperature of the ice and of ſurrounding atmoſphere at the commencement of the experiment was 32°, that of the machine was likewiſe 32°. At the end of the experiment the temperature of the coldeſt part of the machine was near 33°, that of the ice and ſurrounding atmoſphere the ſame as at the commencement of the experiment; ſo that the heat produced by the friction of the different parts of the machine was ſufficient to raiſe the temperature of near half a pound of metal at leaſt one degree, and to convert 18 grains of wax (the quantity employed) into a fluid.

The receiver was now exhauſted. From the exhauſtion, and from the attraction of the carbonic acid gas by the potaſh, a vacuum nearly perfect was, I believe, made.

The machine was now ſet to work. The wax rapidly melting proved the increaſe of temperature.

Caloric then was collected by friction; which caloric, on the ſuppoſition, was communicated by the bodies in contact with the machine. In this experiment, ice was the only body in contact with the machine. Had this ice given out caloric, the water on the top of it must have been frozen. The water on the top of it was not frozen, conſequently the ice did not give out caloric. The caloric could not come from the bodies in contact with the ice; for it muſt have paſſed through the ice to penetrate the machine, and an addition of caloric to the ice would have converted it into water.

Heat, when produced by friction, cannot be collected from the bodies in contact, and it was proved by the ſecond experiment, that the increaſe of temperature conſequent on friction cannot ariſe from diminution of capacity, or from oxydation. But if it be conſidered as matter, it muſt be produced in one of theſe modes. Since (as is demonſtrated by theſe experiments) it is produced in neither of theſe modes, it cannot be considered as matter. It has then been experimentally demonſtrated that caloric, or the matter of heat, does not exiſt.

Solids, by long and violent friction, become expanded,* and if of a higher temperature than our bodies, affect the ſenſory organs with the peculiar ſenſation known by the common name of heat.

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* Expanſion by friction is common to almoſt all bodies; and as the exceptions are very few, it may be admitted as

Since bodies become expanded by friction, it is evident, that their corpuſcles muſt move or ſeparate from each other. Now a motion or vibration of the corpuſcles of bodies muſt be neceſſarily generated by friction and percuſſion. Therefore we may reaſonably conclude that this motion or vibration is heat, or the repulſive power.

Heat then, or that power which prevents the actual contact of the compuſcles of bodies, and which is the cauſe of our peculiar ſenſations of heat and cold, may be defined a peculiar motion, probably a vibration, of the corpuſcles of

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a principle. I have found by experiment, that the metallic ſubſtances, and the ſolid combinations of hydrogen, carbon, and oxygen, become enlarged in all their dimenſions when heated by friction, and I believe all other bodies except ice, in which a new appoſition of particles, and probably a new repulſive motion takes place.

bodies, tending to ſeparate them. It may with propriety be called the repulſive motion.*

Since there exiſts a repulſive motion, the particles of bodies may be conſidered as acted on by two oppoſing forces, the approximating power, which may (for greater eaſe of expreſſion) be called attraction, and the repulſive motion. The firſt of theſe is the compound effect of the attraction of coheſion, by which the particles tend to come in contact with each other, the attraction of gravitation, by which they tend to approximate to the great contiguous maſſes of matter, and the preſſure under which they exiſt, dependant on the gravitation of the ſuperincumbent bodies. The ſecond is the effect of a peculiar motory or vibratory impulſe given to them, tending to remove them farther from each other, and which can be generated, or rather increaſed, by friction or percuſſion. The effects of the attraction of coheſion, the great approximating cauſe, on the corpuſcles of bodies, is exactly ſimilar to that of the attraction of gravitation on the great maſſes of matter compoſing the univerſe, and the repulſive motion is analagous to the planetary projectile force.

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* Heat, in common language, ſignifies that ſenſation which accompanies an increaſe of repulſive motion in any part of our ſyſtem. It ſhould not be uſed therefore for the repulſive motion or cauſe of that ſenſation.

The caloric of the French nomenclators is equally exceptionable; for having been generally uſed to expreſs the imaginary fluid, or matter of heat, it is now aſſociated with, and generally ſuggeſts that idea, and would thus if uſed to expreſs the repulſive motion, or cauſe of heat, become a ſource of error. Words expreſſing compound ideas, ſhould, when formed at will, expreſs as near as poſſible the component parts of theſe ideas, when they are known. The word repulſive motion is, I believe, liable to no exception, I ſhall therefore uſe it to expreſs the ſeparating power of the corpuſcles of bodies, and the cauſe of our ſenſation of heat.

Bodies exiſt in different ſtates, and theſe ſtates depend on the differences of the action of attraction, and of the repulſive power, on their corpuſcles, or in other words, on their different quantities of attraction and repulſion.

When the attraction predominates over the repulſive motion, the body exiſts in the ſtate of ſolidity. In this ſtate its particles are relatively contiguous, conſequently it exiſts in a relatively ſmall ſpace. It is difficultly diviſible by mechanical means; and when divided, its parts being brought into apparent contact, are incapable of aggregative union.

When the ſum of the attractions of all the corpuſcles is nearly equal to that of their repulſive motion, that is, when the attraction and repulſion are in equilibrio, the body exiſts in the ſtate of fluidity. In this ſtate it is difficultly compreſſible, and eaſily diviſible by mechanical means, and when divided, its parts being brought into apparent contact, are capable of aggregate union.

When the repulſive motion predominates over the attraction, the body exiſts in the ſtate of *gazity*, or elaſtic fluidity. Exiſting in this mode, it fills up great ſpace; it is more eaſily diviſible, and more capable of aggregative union than a fluid, and has the peculiar property of elaſticity, that is, of great diminution of volume by compreſſion, and of expanſion by abſtraction of compreſſing forces; ſo that its volume is in the inverſe ratio of the compreſſing weights.

There is another ſtate which has been heretofore unnoticed, and in which only one body that we are acquainted with exiſts, namely light.*

In this ſtate the repulſive motion predominates to ſuch an extent over the attraction, that the corpuſcles indefinitely ſeparate with the greateſt velocity, and appear to be very little acted on by attraction or gravitation. This ſtate may be called repulſive projection.‡

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[*] And probably odorous matters, as they appear to be continually flying off with great velocity from bodies.

[†] As this a ſtate of exiſtence hitherto not particularly noticed, it was thought proper to diſtinguiſh it from other

Every body with which we are acquainted exiſts in one of theſe ſtates, and appears capable of exiſting in either of them by the increaſe or diminution of the repulſive motion of its corpuſcles. But though theſe are modes of exiſtence common to matter in general, yet it appears that all bodies have different quantities of attraction and repulſive motion when exiſting in the ſame ſtate. Different ſolids, fluids, and gaſſes are alike eminently different in their ſpecific gravity, and this difference would induce us to ſuppoſe that the ſums of the attractions and repulſive motions of their particles are different.

When bodies expand, we are certain that their repulſive motion is increaſed; when they contract, we are certain it is diminiſhed; and

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ſtates by a peculiar name. In this ſtate the repulſive motion predominates to ſuch an extent over the attraction, that the particles are projected into ſpace with the greateſt velocity. The term repulſive projection will diſtinguiſh this ſtate from others with ſufficient accuracy, and from mechanical projection.

we have no other infallible teſts of an increaſe or diminution of repulſive motion, but expanſion or contraction.

Bodies may have their repulſive motion increaſed in three modes.

Firſt, By the tranſmutation of mechanical into repulſive motion, that is, by friction or percuſſion. In this caſe the mechanical motion loſt by the maſſes of matter in friction is the repulſive motion gained by their corpuſcles.

Secondly, By the motion of chemical combinations or decompoſition.

Thirdly, From the communicated repulſive motion of bodies in apparent contact.

The diſpoſition in bodies to communicate or receive the repulſive motion, has been called temperature, and the temperature of a body is ſaid to be high or low in proportion as it communicates or receives the repulſive motion. The powers to communicate or receive the repulſive motion, as well as the velocities of communication or reception, are ſpecifically different in different bodies, and as far as we know, do not depend on the abſolute quantities of repulſive motion, but on ſome peculiar atomic conſtitution now unknown to us. As it is found by experiment that the diſpoſition of almoſt all bodies to communicate repulſive motion is increaſed by every addition of it as meaſured by expanſion, and diminiſhed by every ſubtraction as meaſured by contraction, the temperatures of bodies are generally made the meaſures of their relative quantities of repulſive motion.*

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* A peculiar ſenſation known by the name of heat is conſequent on an increaſe of repulſive motion in any part of our bodies, and an oppoſite one called cold on a diminution of it. The common mode of determining the relative quantities of repulſive motion in bodies, is by appealing to theſe ſenſations. Philoſophy uſes the mercurial thermometer. The diſcovery of any mode of accurately determining the contractions and expanſions of ſolids, would be a great acquiſition to ſcience.

Different bodies have their temperatures, or their powers to communicate or receive repulſive motion differently increaſed by the addition, and diminiſhed by the ſubtraction, of equal quantities of repulſive motion. This diſpoſition is called, in the doctrine of caloric, their capacity for heat; but it might be named with greater propriety, their capability of temperature, for it has no relation to the abſolute quantity of repulſive motion they are capable of receiving, but only to their temperature. All bodies, in fact, are capable of any increaſe of repulſive motion, but have their temperatures differently raiſed by this increaſe, that is, have different capabilities of temperature. The body then, that is ſaid to have the greateſt capacity for heat, has the leaſt capability of temperature, and vice verſa. The capability of temperature of bodies is diminiſhed by the addition of repulſive motion, and increaſed by its ſubtraction; ſo that the capability of a body is greateſt when ſolid, leſs when fluid, and leaſt of all when in the gaſeous, or repulſive projectile ſtate.

The capability of temperature of bodies likewiſe depends on the degree of preſſure under which they exiſt. When bodies are compreſſed, their capabilities are increaſed, and their temperatures raiſed; when preſſure is removed from bodies, their capabilities are diminiſhed, and their temperatures lowered.

When bodies in apparent contact communicate the repulſive motion, the motion gained or loſt by one body is uniformly equal to that loſt or gained by the other body, as meaſured by temperature.

When two ſimilar and equal bodies are brought in contact, they acquire a common temperature by communication of their repulſive motion: and the common temperature is an arithmetical mean between the two original temperatures. When two unequal and ſimilar bodies are brought in contact, if their temperatures are different, they will acquire a common temperature by communication, and the communicated repulſive motion will be conſequently divided between them in proportion to their quantities of matter. If two bodies of different capabilities of temperature, and of different temperatures, be brought in contact, they will acquire a common temperature, and the communicated repulſive motion will be found divided between them in proportion to their quantities of matter, and their capabilities.

When bodies combine chemically, it generally happens that the capability of the compound is different from that of either of the conſtituents. Hence in chemical combinations, the capabilities of bodies are either increaſed or diminiſhed. When the capabilities of bodies are increaſed by chemical combination, the temperature of the compound is greater than that of the conſtituents. When the capability of the compound is leſs than that of the conſtituents, its temperature is diminiſhed.

To aſcertain the cauſes of the increaſe or diminution of temperature conſequent on chemical combination, is by far the moſt difficult part of the philoſophy of heat. For we are hardly able to diſtinguiſh the increaſe of temperature in bodies generated by the motion of combination, from that generated by increaſe of capability, and the motion of combination interferes with the diminution of temperature, from diminution of capability; and chemical proceſſes are in general ſo complex, that we cannot diſtinguiſh between the increaſe of repulſive motion from compoſition, and that ariſing from decompoſition.

We have every reaſon to ſuppoſe from the foregoing experiments, and obſervations that the repulſive power of bodies is a peculiar motory impulſe. To diſtinguiſh this motion from others, and to ſignify the cauſe of our ſenſations of heat, &c. the name *repulſive motion* has been adopted.

On the theory of caloric, a peculiar elaſtic fluid was ſuppoſed to exiſt between the particles of all bodies; and the peculiar modes of exiſtence of bodies, that is, ſolidity, fluidity, and gazity, were ſuppoſed to depend on the different quantities of caloric entering into their compoſition. The elaſticity, that is, the compreſſibility of this fluid, has been univerſally admitted, and the compreſſibility of the gaſes has been ſuppoſed to depend on the compreſſibility of their caloric.

Now ſince Caloric is ſuppoſed compreſſible, that is, capable of having its volume diminiſhed by preſſure, its particles cannot be in actual contact; there muſt conſequently act on them ſome power which prevents their actual contact, that is, the repulſive motion. So that to admit the exiſtence of an imaginary fluid in conformity to the abſurd axiom, *bodies cannot act where they are not*, is in fact the ſolution of a ſmall difficulty by the creation of a great one. After all, a principle muſt be admitted, (that is, repulſion ;) to do away the neceſſity of which, caloric has been invented.

On the theory of repulſive motion, it is evident that the gaſes (which M. Lavoiſier and the French nomenclators have aſſumed to be ſimple ſubſtances combined with caloric) muſt be either ſimple ſubſtances in the ſtate of elaſtic fluidity, or combinations of two or more ſimple ſubſtances. Hydrogen and nitrogen gas we have not yet been able to decompoſe; they are then relative to the preſent ſtate of our knowledge, ſimple ſubſtances. Oxygen gas, as will be hereafter proved, is compoſed of light and oxygen.

Since the word gas adopted by the French nomenclators is intended to expreſs the chemical combination, or rather the ſaturation of bodies with caloric, it is doubtless exceptionable. The following arguments will, I think, prove that it ought not to remain in the chemical nomenclature.

For firſt, Bodies, when rendered from ſolids to fluids, from fluids to gaſes, are not eſſentially altered, their corpuſcles are farther ſeparated, that is, they move in greater ſpace than before; but the body is equally ſimple, it has undergone no decompoſition or combination; the word then that expreſſes the chemical combination of bodies with caloric is improper.

Secondly, All bodies with which we are acquainted have certain quantities of repulſive motion; and they have different modes of exiſtence dependent on their quantities of repulſive motion. The moſt common of theſe modes are ſolidity, fluidity, and gazity; in each of theſe ſtates the bodies are equally ſimple. That ſtate in which they are found at the common temperature of the atmoſphere, is the ſtate from which they derive both their common and philoſophic names. Now the French nomenclators have called all ſimple ſubſtances (the gaſes excepted) by their common names, without making any alteration to expreſs their combination with caloric. They have called all the metals, which are capable of exiſting like other bodies, in three ſtates, by their common names; and on their own principles theſe bodies are combined with caloric. They ſhould therefore have diſtinguiſhed them by names expreſſing this combination, and have called gold, mercury, and ſulphur, ſolid gold, fluid mercury, and ſolid ſulphur, for the ſame reaſons that they have given the names of hydrogen and azotic gas to the mephitic and inflammable airs, which are probably metals in the ſtate of elaſtic vapor.

Thirdly, Thoſe ſubſtances which have been called gaſes, uniformly exiſt in the ſtate of elaſtic fluidity, at the common temperature of our planet. Simple names, without the addition of gas, would diſtinguiſh them from all other ſubſtances. With the ſame propriety that we uſe the term hydrogen gas in chemiſtry, we might make uſe of the terms, ſolid gold, fluid mercury, and fluid alcohol. In treating of the changes made in bodies by the repulſive motion, we may with propriety uſe the terms, ſolid, fluid, and gaſeous, to expreſs the different modes of exiſtence of the ſame body. But ſimple ſubſtances ſhould be diſtinguiſhed by names characteriſtic of their properties; compound ſubſtances ſhould be diſtinguiſhed by names expreſſive of the combination, that is, of the ſubſtances forming the compound. In conformity to theſe principles, I ſhall omit any names ſignifying the peculiar modes of exiſtence of bodies; treating of ſubſtances, I ſhall give them their ſimple names, and by theſe names I mean to expreſs the ſtate in which they exiſt at the common temperature of the atmoſphere. As for example, in uſing the words gold, mercury, and hydrogen, I mean ſolid gold, fluid mercury, and gaſeous hydrogen. Oxygen gas, (which the French nomenclators have aſſumed to be oxygen combined with caloric) will be proved to be a ſubſtance compounded of light and oxygen. It would be highly improper to denote this ſubſtance by either of the terms oxygen gas, or oxygen. The one would ſignify that it was a ſimple ſubſtance combined with caloric, the other that it was a ſimple ſubſtance, the acidifying principle. The term *phoſoxygen* (from φως light, οξυς acid, and γενητωρ generator) will I think be unexceptionable; it will expreſs a chemical combination of the ſimple ſubſtance light, with the ſimple ſubſtance oxygen ; it will not materially alter the nomenclature of the French philoſophers ; and as will be ſeen hereafter, it can be eaſily modified to expreſs, in conjunction with other words, the combinations of light and oxygen.

Of LIGHT.

Light is a body in a peculiar ſtate of exiſtence. Its particles are ſo amazingly minute, that they are very little affected by gravitation; and paſs unaltered through the pores of diaphanous bodies. They move through ſpace with a velocity almoſt inconceiveable, and communicate no perceptible mechanical motion to the ſmalleſt perceptible particles of matter.* From the peculiar velocity of light we eſtimate its quantity of repulſive motion. The influence of the attraction of gravitation on light is very ſmall, as is evident from its not apparently gravitating towards the ſun or the earth. The influence of the coheſive attraction on its particles is likewiſe very ſmall, as is evident from their uniform ſeparation, &c.; but the repulſive motion acting on the corpuſcles of light is very great, as is apparent from their velocities, and continual ſeparating motion. But as we have ſaid before, the diſtances of the corpuſcles of bodies from each other, and the velocities of their motions, are in a ratio compounded of their repulſive motion and attraction. When the repulſive motion eminently predominates over the coheſive and gravitative attraction, the particles of matter will indefinitely ſeparate, as thoſe of light. To diſtinguiſh this ſtate of exiſtence, peculiar to light, from thoſe other bodies, we have given it the name of *repulſive projection.*

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* It will be by and by proved that they communicate portions of their repulſive motion to the corpuſcles of bodies.

Light is the ſource of the moſt numerous and pleaſurable of our perceptions. This tribe of perceptions is thus received; particles of light in the ſtate of repulſive projection coming in contact with the retina, communicate to it portions of their repulſive motion. The retina appears to be compoſed of nervous medulla and as ſome ſuppoſe of irritable fibre. The communicated motion of light either ſtimulates the irritable fibre into contraction, which contraction is accompanied with that affection of the nerve correſponding to a ſenſation: or the motion of light communicated to the nerve itſelf produces the ſenſorial affection. The former of theſe opinions is rendered probable by the experiments of Dr. Darwin on ocular ſpectra.* It is then neceſſary to our perceptions of light, that it exiſt in its peculiar ſtate of repulſive projection; we conſequently cannot perceive it by viſion in any other mode of exiſtence.

It appears from experiment that our ſenſations of viſion are occaſioned by the united impulſe of a number of particles of light both ſynchronically and ſucceſſively falling on the retina. All our different ſenſations then muſt ariſe from differences in the particles of light, their motions and numbers, or from differences in ſome of theſe.

Light paſſing through diaphanous bodies, is attracted by their particles, and the attraction of the particles of bodies for light is proportional to their denſity and combuſtibity. The laws of the attraction of diaphanous bodies for light are the laws of refraction which have been ſo admirably explained by the immortal Newton. He diſcovered that the particles of ſolar light are not equally attracted in paſſing though different bodies: they are differently refracted in paſſing through the priſm, and ſeparated into ſeven claſſes of particles that produce the ſenſations of red, orange, yellow, green, blue, indigo, violet. This difference is eaſily accounted for, by ſuppoſing that the particles of light in taking the ſtate of repulſive projection, originally received a different repulſive motion. The red particles being ſuppoſed to vibrate with the greateſt velocity, muſt be leaſt attracted by the particles of the medium through which they pass; the violet particles moving with the leaſt velocity, muſt be moſt refracted, and the different velocities of the intermediate coloring particles being ſuppoſed, their different refractions muſt be correſpondent, which is found to be the caſe.

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* See the end of Darwin's Zoonomia, Vol. 1.

Light is reflected from bodies that it cannot penetrate or combine with. The ſenſations we receive from reflected light are eminently different. A correſpondence has been long obſerved between the colors of bodies, and the increaſe of repulſive motion in them from the action of light. This correſpondence is a ſubject of great importance to phyſical ſcience, and worthy of inveſtigation. Heat has been proved to be a peculiar repulſive motion of the particles of bodies. Light is a body, the particles of which are acted on by the greateſt repulſive motion. When a body has its repulſive motion increaſed by the action of light, a portion of the repulſive motion of light muſt be loſt, a portion equal to that gained by the body acted on; this is evident from what has been ſaid on the communication of repulſive motion in the doctrine of heat.

To determine accurately the correſpondence between the increaſe of repulſive motion in bodies from the action of light, and their colors, I made the following experiment.

EXPERIMENT IV.

Six ſimilar pieces of copper,* of equal weight, ſize, and denſity, were thus colored, one white, one yellow, one red, one green, one blue, and one black. A portion of a mixture of oil and wax, which became fluid at about 76°, was placed on the centre of each on the inferior ſide. They were then attached to a board painted white, and ſo placed with regard to the ſun, that their upper ſurfaces were equally expoſed to the light. Their inferior ſurfaces, to which the cerate was attached, were equally deprived of light and heat, that is, they were ſo expoſed, that there could be no miſtake with regard to the repulſive motion generated in them by the action of light. The changes of temperature in them from the action of light, took place in the following order. The cerate on the black plate began to melt perceptibly before the reſt, the blue next in order, then the green and the red, and laſtly the yellow;

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* Each a cubic inch ſquare, and two lines thick.

the white was ſcarcely at all affected when the black was in a complete ſtate of fuſion.

This experiment proves that the increaſe of repulſive motion in bodies from the action of light, is great in proportion as the colors are dark. Now as our ſenſations ariſe from the united impulſe of a number of particles on the retina, in proportion as the vibratory motions of theſe particles are greater or leſs, ſo in proportion muſt our ſenſations be different.

It does not appear that reflected light is in any inſtance compoſed of particles that have equal quantities of repulſive motion; the differences of ſenſations therefore moſt probably ariſe from the differences occaſioned in the vibrations of all the particles, from their communicating to the reflecting bodies portions of their repulſive motion, and not from the communication of the greater portion of that of ſome of the particles, that is, of thoſe which are ſuppoſed in the Newtonian ſyſtem to be abſorbed. On this ſuppoſition the light reflected from white bodies (which may be ſaid to have the leaſt capability of ſubtracting the repulſive motion of light) muſt vibrate with the greateſt velocity, that is, muſt be reflected nearly unaltered. The particles reflected from dark bodies (which may be ſaid to have the greateſt capability for receiving the repulſive motion of light) communicating great portions of their repulſive motion, muſt vibrate with the leaſt velocity, and all the intermediate colors may depend on the different velocities of vibration. Their vibrations, though of different lengths, may be iſochronous, and all the particles may paſs through rectilineal ſpace in equal times.

Bodies perfectly black muſt ſubtract ſo much of the repulſive motion of light, as to deprive it of its repulſive projectile form. The electric fluid is probably light in a condenſed ſtate, that is, not ſupplied with the repulſive motion ſufficient to give it repulſive projection. Its chemical action upon bodies is ſimilar to that of light, and when ſupplied with repulſive motion by friction, or the contact of bodies from which it is capable of ſubtracting it, it takes the repulſive projectile form, and becomes perceptible as light. It is extremely probable that the great quantity of this fluid almoſt every where diffuſed on our earth, is produced from the condenſation of light, from the ſubtraction of its repulſive motion by black and dark bodies. This fluid continually formed from the condenſation of light, is probably again ſupplied with repulſive motion at the poles, by the revolution of the earth on its axis, and given off in the form of repulſive projectile light, whilſt a quantity equal to that given off from its equilibrating principle is ſupplied continually from the other parts of the globe. Hence the phænomenon of the aurora borealis, or northern lights. No more ſublime idea can be formed of the motions of matter, than to conceive that the different ſpecies are continually changing into each other. The gravitative, the mechanical, and the repulſive motions, appear to be continually mutually producing each other, and from theſe changes all the phænomena of the mutation of matter probably ariſe.

Though the temperature of the atmoſphere, which is found proportional to the light paſſing through it, may in ſome meaſure depend on the repulſive motion communicated to it by opake colored bodies; yet it is reaſonable to ſuppoſe that the particles of light in paſſing through the atmoſphere, loſe ſmall portions of their repulſive motion, which is the great cauſe of the atmoſpheric heat. Water, glaſs, and other tranſparent bodies, are capable of having their repulſive motion increaſed by the action of light; and light in paſſing through them becomes colored independent of decompoſition. A body perfectly white appears colored in deep water, and Dr. Halley, when in the diving bell, found that his hand, expoſed to the ſolar light, became of a deep red. Is not the blue color of the air a proof that the repulſive motion of light is diminiſhed in paſſing through it? May not the atmoſpheric temperature, and the refraction of light in it be in a great meaſure owing to the water held in ſolution by the air, for the temperature is lower in proportion as we advance higher in the atmoſphere, and refraction does not take place above forty-five miles high, and we are certain from the phænomena of fiery meteors, that the atmoſphere extends at leaſt as high again.

On the above mentioned ſuppoſition, all our infinitely different ſenſations from reflected light muſt ariſe from differences in the numbers and repulſive velocities of the particles, and all light, if we may appeal to our ſenſations, muſt be in ſome meaſure altered both by reflection and refraction.

Bodies that do not contain light in combination, and that are incombuſtible, under certain circumſtances become luminous.

To diſcover the cauſe of the luminous appearance of incombuſtible bodies, it is neceſſary to conſider the circumſtances under which it takes place. When glaſs, ſilex, argil, a metallic oxyd, or any other incombuſtible body, is expoſed to a ſtrong light, as the focus of a lens, its temperature is gradually raiſed, that is, its repulſive motion is increaſed. After a certain time it become luminous, white, or red hot: and if it be now removed out of the focus, it continues for ſome time to give out light, and to communicate repulſive motion to the ſurrounding bodies, till the equilibrium of temperature is reſtored.

Now when bodies have their repulſive motion increaſed by the action of light, it is evident that the motion gained by the body, is that loſt by the light; and as a certain quantity of repulſive motion is eſſential to its repulſive projection, it muſt after conſiderable communication of repulſive motion, ceaſe to exiſt in that ſtate. In its new ſtate of exiſtence it is probably not perceptible to any of our ſenſes. We therefore in this caſe can only reaſon from its effects. The body continues for ſome time to give off light, after being removed out of the focus; light therefore muſt have been condenſed in ſome form around it, and being gradually ſupplied with repulſive motion from the body, flies off ſlowly in the repulſive projectile form, becomes perceptible as light, and is the cauſe of the peculiar ſenſation known by the name of red or white heat.*

From what has been ſaid, it is evident that light, both in the ſtate of repulſive projection, and in the form of the electric fluid, performs an important part in the phyſical phænomena of the univerſe. In any other ſtates than theſe, we have not hitherto been able to detect it by the ſenſes; but we know not what we may be enabled to accompliſh by means of a more extenſive and philoſophic chemiſtry. A number of the elements are already in our power, and the rapid progreſs of the perfection of ſcience ſeems to promiſe us the knowledge of thoſe etherial fluids, which at preſent elude the perception of our organs, and are only known by their effects.

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* This theory of the luminous appearance of incombuſtible bodies may be compared with Macquer's and Newton's.. Fourcroy ſays, " L'incandeſcence des corps incom-" buſtibles, telles que les pierres dans leſquelles on ne peut " point admettre la preſence de la lumiere combinée, au " moins comme dans les corps incombuſtibles, a été expli-" quée d'une maniere tres ingeniuſe par Macquer. Suivant " ce chemiſte, elle depend des vibrations fortes, excitees. " dans les molecules de ces corps par la chaleur ; ces vibra-" tions diſpoſe les particles de ſorte que leur facettes, ſans " ceſſe agitees ſont autant de petit miroirs qui reflechiſſent " vers nos yeux les rayons de lumiere, qui exiſtent dans " l'air pendant la nuit autant que pendant le jour, et qui " ne ſont inſenſibles, et ne produiſent les tenebres que " parceque leur direction ne ſe fait pas ſur les organes de la " vue.

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The immortal Newton ſays, " Do not all bodies when " heated beyond a certain degree, emit light and ſhine, " and is not this emiſſion performed by the vibratory mo-" tions of their parts," &c. Sec 8, 9, and 10 queries at the end of his optics. The firſt experiment appears to overturn the foundation of theſe theories.

The chemical effects of light are not leſs important than the phyſical. Its combinations, hitherto almoſt unnoticed, have the higheſt connection (as will be ſeen hereafter) with organic exiſtence; and the moſt aſtoniſhing and beautiful of the chemical phænomena depend upon them.

Light enters into the compoſition of a number of ſubſtances. In ſome of theſe, the incombuſtible phoſphoreſcent bodies, it moſt probably exiſts in a ſtate of looſe combination, and the preſence or abſence of light does not appear to alter their properties materially. In *phoſoxygen* (oxygen gas) it is intimately combined with oxygen.

Of the COMBINATIONS *of* LIGHT.

Of PHOSPHORESCENT BODIES.

Certain bodies, after being for ſome time expoſed at a high temperature to light, continue luminous for a conſiderable length of time after this expoſure. Such are many preparations of lime, the bolognian ſtone, &c. This phænomenon is in ſome meaſure analogous to the ignition of incombuſtible bodies.

Light, it appears, is only ſuſceptible of combining, and of remaining in combination with thoſe bodies at a higher temperature than that of our atmoſphere; at the common temperature it is liberated.

The name of ſolar phoſphori has been given to theſe bodies, in common with all others that become luminous, independant of combuſtion.

Other bodies exiſt, which become luminous when their repulſive motion is increaſed by communication of it from ſome bodies of a higher temperature. Light remains in combination with theſe bodies only at a low temperature. When their repulſive motion is increaſed, the light is liberated. This decompoſition appears to ariſe from the diminution of the chemical attraction between light and the body, by the repulſive motion, and from the ſupply of a quantity of it ſufficient to enable light to fly off in the repulſive projectile form. Amongſt theſe bodies are the different combinations of lime and particularly the fluate (the colors of which appear to depend upon combined light), different combinations of barytes, the ſulphate of potaſh, ſome of the metallic oxyds, cotton, wool, oils, wax, alcohol, &c. We owe the diſcovery of the greater number of theſe phoſphoreſcent bodies to Mr. T. Wedgwood.*

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* See his ingenious paper in the phil. tranſ. for 1792.

We have found that the ſulphate of ſtrontian, on an increaſe of temperature, gives out a pale light.

There is a claſs of phoſphoreſcent bodies, which give out their combined light on attrition. Amongſt theſe are borate of ſoda, ſulphate of argil, tartrite of potaſh, and all the ſilicious claſs of ſtones.§ This phoſphoreſcence may be accounted for in the ſame manner as the laſt ſpecies.

Certain ſubſtances give out their combined light on immerſion into the mineral acids. When magneſia* is thrown into the ſulphuric acid, a light is liberated which produces a ſenſation ſimilar to that known by the name of red heat. The ſame effect is produced when the nitric acid is uſed.

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§ For a further account of theſe bodies, ſee the ſame paper of Mr. T. Wedgwood.

* I was informed of this phænomenon by Dr. Beddoes, who had previouſly witneſſed it ſeveral times.

During the combination of lime with the mineral acids, a flaſh of white light is uniformly perceived; the ſame effect is not produced during the combination of ſtrontian and barytes with theſe acids.

This phænomenon appears to be owing both to the attraction of the acids, and to the repulſive motion generated during the combination, a motion ſufficient to give to the combined light, repulſive projection; for lime and magneſia become luminous when heated, which is not the caſe with ſtrontian and barytes.

It is probable that ſome of the combuſtible bodies are phoſphoreſcent. From an experiment of the Dutch chemiſts it appears that ſulphur in its common ſtate contains light. This experiment, which has been the ſubject of much ſpeculation, has been ſeveral times repeated by Mr. Clayfield,* (and once in my preſence) with reſults different from thoſe mentioned by the diſcoverers. When copper and ſulphur cautiouſly freed, and included from phoſoxygen (oxygen gas) were made to combine by the heat of an argand lamp, a luminous appearance was perceived during the combination, and a conſiderable quantity of an incombuſtible gas, mingled with ſulphureous acid, was liberated.

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* It is to be hoped that this ingenious chemiſt will ſoon publiſh a particular account of it.

Phoſphorus appears to contain light. Mr. Clayfield has often made me obſerve, during the combination of phoſphorus and the earths, a fine vivid light, wherever the phoſphoric vapor came in contact with the earths heated red.

The ſuppoſition that the combuſtible bodies are phoſphoreſcent, may in ſome meaſure reconcile the phlogiſtic and pneumatic theories with each other.

The phoſphoreſcence of certain inſects and putrifying animal ſubſtances will be treated of in the theory of reſpiration.

All the above mentioned phænomena equally take place in any gas, and appear to be perfectly independent of combuſtion.

Of PHOSOXYGEN, *or* OXYGEN GAS.

This gas (firſt diſcovered by the immortal Prieſtley) the great Lavoiſier ſuppoſed to be oxygen combined with caloric, and on this ſuppoſition his theory of combuſtion is founded. The non-exiſtence of caloric, or the fluid of heat, has been proved, and the materiality of light demonſtrated.

Light is liberated during the oxygenation of certain bodies, as the following experiments will prove.

EXPERIMENT V.

The repulſive motion of carbon in contact with phoſoxygen (oxygen gas) was increaſed by a burning glaſs, till it became luminous; the carbon was rapidly diminiſhed with the liberation of a great quantity of light; the temperature of the glaſs globe containing the phoſoxygen (oxygen gas) was very much increaſed, and a very ſmall diminution of weight* was perceived. The phoſoxygen (oxygen gas) and carbon were almoſt entirely conſumed, and a quantity of carbonic acid was formed, apparently nearly equal to the carbon and phoſoxygen.

EXPERIMENT VI.

The temperature of phoſphorus in contact with phoſoxygen was raiſed by a burning glaſs, it immediately became luminous. An immenſe quantity of light was liberated, and the temperature of the ſurrounding bodies very much increaſed. A deficiency of weight was obſerved, a deficiency more conſiderable than I have found in any other combuſtible proceſs; and phoſphoric acid nearly equal to the phoſoxygen and phoſphorus was formed.

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* The following mode of aſcertaining the diminution of weight was adopted in theſe experiments. The combuſtible body was ſuſpended in a ſmall earthen cup, in a glaſs globe filled with phoſoxygen. This globe was heated to dry it perfectly, attached to a ballance, and accurately weighed before combuſtion. The combuſtible body was fired by a burning glaſs. During combuſtion the globe always rapidly aſcended from the rarefaction of the ſurrounding atmoſphere. After combuſtion, when the common temperature was reſtored, there appeared an evident ſmall diminution of weight. This diminution was too ſmall to be exactly aſcertainable by the ballance I made uſe of, and appeared to be different in different proceſſes.

EXPERIMENT VII.

Sulphur was heated in phoſoxygen. It rapidly diminiſhed with the liberation of a great quantity of light, and great increaſe of temperature in the ſurrounding bodies. After combuſtion a ſmall deficiency of weight was found; and ſulphuric acid nearly equal to the ſulphur and phoſoxygen employed, was formed.

EXPERIMENT VIII.

The temperature of hydrogen in contact with phoſoxygen was raiſed. The gaſes were diminiſhed with great liberation of light, and great increaſe of temperature; and water nearly equal to them was formed.

EXPERIMENT IX.

Zinc was heated in contact with phoſoxygen; it became luminous, and was conſumed with a white brilliant flame. The ſubſtance remaining after combuſtion was white oxyd of Zinc.

EXPERIMENT X.

A ſmall gun lock, armed with an excellent flint, was ſnapped in a veſſel filled with phosoxygen. The ſparks of light ariſing from the particles of ſteel ſeparated by colliſion, were the moſt brilliant that can be imagined; and theſe particles examined by a magnifier, were found converted into black oxyd of iron.*

From theſe experiments, it appears that in the chemical proceſs of the formation of many oxyds and acids, light is liberated, the phoſoxygen and combuſtible baſe conſumed, and a new body formed, with properties eſſentially different from the ſubſtances entering into the combination. Since light is liberated in theſe proceſſes, it is evident, that it muſt be liberated either from the phoſoxygen, or from the combuſtible body. It appears, as we have before ſaid, that ſmall portions of light are contained in ſome of the combuſtible bodies; but it appears to be accidental, and common to them with incombuſtible bodies; for their properties are not apparently altered when it is driven from them by increaſed repulſive motion; and we have no reaſons for ſuppoſing that carbon, hydrogen, or any of the metals contain the ſmalleſt portions of light.

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* This experiment compared with the firſt, will afford a ſtrong proof, not only of the compoſition of phoſoxygen, but likewiſe of the doctrine of repulſive motion.

If the light liberated in combuſtion be ſuppoſed (according to Macquer's and Hutton's theories) to ariſe from the combuſtible body, then phoſoxygen muſt be conſidered as a ſimple ſubſtance; and it follows on this ſuppoſition, that whenever phoſoxygen combines with combuſtible bodies, either directly or by attraction from any of its combinations, light muſt be liberated, which is not the caſe, as carbon, iron, and many other ſubſtances, may be oxydated by the decompoſition of water, without the liberaton of light.

Theſe experiments will appear more concluſive when the ſynthetic experiments are conſidered, and the whole theory examined. It appears that whenever bodies ſimply combine with oxygen, light is liberated. There are bodies that combine with oxygen and portions of light, as will be hereafter proved, and others that combine with phoſoxygen.

Combuſtion is a complex chemical proceſs. The decompoſition of phoſoxygen by the attraction of a body for oxygen. The light is generally liberated in the repulſive projectile form, and the oxygen combines with the attracting body to form an oxyd or acid. The great increaſe of temperature ariſes from the diminution of capacity in the combining bodies, from the repulſive motion generated during the combination, and from the concentrated liberated light.

It is probable that there are ſome decompoſitions ſo flow, that the generated repulſive motion is not ſufficient to give to light repulſive projection. A few metallic oxydations appear to be of this nature.

The light liberated in different combuſtive proceſſes, aſſumes very different appearances. During the combuſtion of bodies that remain ſolid or fluid at the temperature of combination, the ſenſation given by the liberated light is that which has been called a red or white heat. If the combuſtion proceeds but ſlowly, the red heat is produced, if more rapidly, the white. In the combuſtion of bodies that are gaſeous, at the temperature of combination, the ſenſation known by the name of flame is produced by the liberated light. Theſe different ſenſations depend, moſt probably, on the rapidity of combuſtion. The light muſt be liberated ſlowly when phoſoxygen is decompoſed by ſolids or fluids, and much more rapidly in the decompoſition of gaſes.

The differences in the colors of the liberated light, muſt ariſe from different repulſive velocities given to the particles.

Since light and heat are totally diſtinct, it is evident that the names red and white heat, are improper, as applied to different modifications of light. Philoſophy demands a more unequivocal nomenclature. The red and white heat might be called red and white ſlowly liberated light. As flame is a ſingle word that can ſignify nothing but a modification of light, it may with propriety be retained in phyſical language.—To explain the phænomena of combuſtion, on their theory, the phlogiſtians were obliged to conſider all combuſtible bodies as combinations of different unknown ſimple ſubſtances, with the undemonſtrated phlogiſton; and phoſoxygen, or vital air, a ſimple ſubſtance. This theory tended to confuſe ſcience, by referring to many unknown ſubſtances, to account for phænomena which evidently depend upon known ones. The phænomena of combuſtion, and the generated increaſe of temperature, are eaſily explained on the theory of repulſive motion, and on that of the compoſition of phoſoxygen, and only one unknown principle is admitted, namely oxygen, which we have never been able to obtain in its ſimple ſtate, on account of its ſtrong attraction for light and other ſubſtances ; but whoſe exiſtence is perfectly demonſtrable.

The theory of the immortal Lavoiſier and the ingenious French nomenclators will ever be admired by chemical philoſophers. It appears, indeed, to be poſſeſſed of theſe defects alone : the aſſumption of the imaginary fluid caloric, and the total neglect of light. According to the caloriſts, combuſtion ought always to take place when the gaſes are condenſed ; but it is found that none of the gaſes, (except thoſe of which phoſoxygen is a component part) however rapid their combinations or decompoſitions, produce the ſmalleſt combuſtion, or liberation of light, when ammonia and carbonic acid (whoſe united capacity is much greater than that of phoſoxygen and phoſphorus) combine, no combuſtion is produced, and the increaſe of temperature is leſs than might be expected from ſo great a contraction of volume.

The proofs of the compoſition of phoſoxygen founded on ſynthetical experiments, are no leſs concluſive than thoſe deduced from the analytical ones. *It will appear that the preſence of light is abſolutely eſſential to the production of phoſoxygen from pure oxyds and acids.*

Of thoſe ſubſtances that have been heretofore diſtinguiſhed by the common name of oxygen attractors, there are ſome, as has been before ſaid, that wholly decompoſe phoſoxygen by attracting the oxygen, with which they combine, whilſt the light is liberated. Theſe oxyds are difficult of decompoſition by light alone, as is reaſonable to ſuppoſe, and if at all, at a much higher temperature than of that of their combination with oxygen. The oxyd of lead is decompoſable with leſs light, and with leſs increaſe of repulſive motion, than probably any of the other oxyds, as will appear from the following obſervation.

OBSERVATION *a.*

When pure oxyd of lead is heated as much as poſſible included from light, it remains unaltered; but when expoſed to the light of a burning glaſs, or even of a candle, phoſoxygen is generated, and the metal revivified.

In this proceſs it is neceſſary that the temperature of deoxydation be greater than that of oxydation, as oxygen, at a certain temperature, has a ſtronger attraction for lead than for light; but at a higher temperature, it attracts light ſtronger than lead.

But few experiments have heretofore been made on the revivification of metallic oxyds by the ſimple application of heat and light. In the common proceſſes, they are reſtored by placing them in contact with bodies that have a ſtronger attraction for oxygen.

From the obſervation on the pure oxyd of lead, and thoſe which have been made on the other oxyds, it appears that light is abſolutely eſſential to the generation of phoſoxygen from pure oxyds, and that phoſoxogen is never produced from them, but when light is preſent.

The ſubſtances that partially decompoſe phoſoxygen, that is, that combine with oxygen and portions of light, are more eaſily decompoſable by light. The attraction between the baſe and oxygen is weakened by the attraction of light for oxygen; and the addition of a ſmall quantity of light effects the decompoſition; phoſoxygen is formed, and the phoſoxydable baſe remains pure, as the following obſervations will prove.

OBSERVATION *b.*

Oxygenated muriatic acid is a compound of muriatic acid, oxygen, and light, as will be hereafter proved. The combined light is not ſufficient to attract the oxygen from the baſe to form phoſoxygen; but its attraction for oxygen renders the acid eaſily decompoſable. If this acid be heated in a cloſe veſſel, and light excluded, no phoſoxygen is formed; but if it be expoſed to the ſolar light, phoſoxygen is formed, the acid loſes its oxygen and light, and becomes muriatic acid.

Now ſince light, by producing repulſive motion cannot, as is evident from the firſt part of the laſt experiment, decompoſe oxygenated muriatic acid, it is evident that it muſt act chemically, that is, by combination; it muſt attract oxygen and light from the acid, and this combination is phoſoxygen.

OBSERVATION *c.*

If nitric acid, which is compounded of oxygen, light, and nitrogen, (as will be hereafter proved) is expoſed to the ſolar light, phoſoxygen is formed, and the acid reduced to the ſtate of nitrous acid, that is, is deprived of a portion of its oxygen and light.

OBSERVATION *d.*

The yellow oxyd of tungſten conſiſts of a peculiar metallic baſe, oxygen, and probably a ſmall portion of light. If this oxyd be expoſed to the ſolar light, phoſoxygen is produced, the oxyd loſes weight, and becomes blue.

OBSERVATION *e.*

The green pruſſiate of iron, expoſed to the ſolar light, gives out phoſoxygen, and becomes blue.

OBSERVATION *f.*

If the oxyds of gold or ſilver be expoſed to the ſolar light, phoſoxygen is produced, and the metals deoxydated.

From theſe obſervations it appears that light is eſſential to the production of phoſoxygen from oxyds and acids; and the quantity of light eſſential, appears to be inverſely proportional to the quantity contained in the combination. The ſubſtances that contain portions of light combined with their oxygen, are eaſily revivified by a ſmall quantity of light, and a ſmall increaſe of repulſive motion. The compounds, on the contrary, that are pure oxyds, that is, which contain no light, as the oxyds of iron, nickel, &c. require large portions of concentrated light, and a great increaſe of temperature, to produce from them phoſoxygen.

Certain combinations of oxygen cannot be decompoſed by the ſimple elective attraction of light for oxygen. Theſe require for their decompoſition the united force of two attractions: that of light for oxygen, and of ſome ſubſtance for the oxydable baſe.

Among theſe ſubſtances are water and carbonic acid.

Water, as is proved by the tenth experiment, is compoſed of oxygen and hydrogen. When the oxygen of phoſoxygen combines with hydrogen to form water, light is liberated. Water is decompoſed by two attractions; that of light for oxygen, and of a certain hydrogen attractor for hydrogen.

The marine cryptogamiæ, vegetables, and a number of other ſubſtances, attract hydrogen.

That the marine cryptogamiæ* attract hydrogen, is evident from their analyſis. I have found that they afford, when decompoſed by repulſive motion, amongſt other products, a conſiderable quantity of hydrogen. This hydrogen, as they are nouriſhed entirely by water, or by ſubſtances held in ſolution by water, it is reaſonable to ſuppoſe they ſomehow gain from the decompoſition of water. To remove all doubt, however, concerning the attraction of the marine cryptogamiæ for hydrogen, I made the following experiment.

EXPERIMENT XI.

One cubic inch of conferva fæniculacea was put into a veſſel containing thirteen cubic inches of hydrogen. It remained in a temperature of 58° for ſix hours, and at the end of that time was examined. The hydrogen was diminiſhed eight tenths of a cubic inch. I could get no ballance ſufficiently accurate to determine the weight gained by the plant.

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* I have prefered giving an account of the decompoſition of water by the marine cryptogamiæ, to that effected by the vegetation of land plants, both as it is a fact heretofore unknown, and as, from the inferiority of their organic powers, their chemical attraction may be more readily admitted.

As the marine cryptogamiæ cannot be obtained perfectly dry in their vegetative ſtate, it was neceſſary to prove that the hydrogen gas diminiſhed was not abſorbed by the water in contact with the plant. For this purpoſe two phials, containing each 13½ cubic inches, were filled with hydrogen. One cubic inch of conferva fœniculacea was inſerted into the one, and two cubic inches of wool, previouſly wetted, into the other. The quantity of gas in each of them was then accurately determined. The phials were inverted in the ſame veſſel of water, and at the end of twelve hours examined. The diminution of the conferva was near a cubic inch and quarter; by the wool and water, not more than three-tenths of an inch.

This experiment proves that the marine cryptogamiæ attract hydrogen; but their attraction is weaker than the attraction of oxygen ſor hydrogen, as it is found that ſeaweeds, or vegetables of any kind, placed in contact with water, and deprived of light, effect no alteration in it as long as they retain life. Water, conſequently, is not decompoſed by the ſimple elective attraction of bodies for its hydrogen.

The attraction of a body for hydrogen does not effect the decompoſition of water, when aſſiſted by an increaſe of repulſive motion, as the following experiment will prove.

EXPERIMENT XII.

One hundred and four cubic inches of water, previouſly boiled to expel the atmoſpheric air, were heated with three cubic inches of conferva fuſca, in a veſſel from which light was excluded. The temperature was gradually raiſed to 200°; but not more than a few globules of gas were formed, which by trial with nitrous gas proved to be of the ſame quality as atmoſpheric air.

Water expoſed to the ſolar light in contact with the marine cryptogamiæ, or any organized hydrogen attractor, is decompoſed by the force of two attractions; that of the hydrogen attractor for hydrogen, and of light for oxygen, as the following experiment will prove.

EXPERIMENT XIII.

Into a green glaſs globe containing 214 cubic inches of ſea water, previouſly boiled to expel the atmoſpheric air, one cubic inch of conferva littoralis was inſerted. The globe was inſerted in a jar of water of a ſimilar kind, and expoſed in a bright ſunſhine for four hours. In this time five cubic inches of gas were formed; which by trial with nitrous gas, proved to be $\frac{71}{100}$ parts phoſoxygen, and $\frac{29}{100}$ azote. * The next day the globe was expoſed to a bright ſunſhine for three hours. Two cubic inches, and five tenths were pro-

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* By azote, I mean a gas incapable of diminution with nitrous oxyd; I have always called the gas to which the French nomenclators give the name of azote, nitrogen, after Chaptal, and ſome Engliſh chemiſts.

duced, which by trial with nitrous gas, proved to be $\frac{76}{100}$ phoſoxygen, and $\frac{24}{100}$ azote. The day after, the globe was expoſed for five hours; but the ſky was often clouded, and there fell ſome ſhowers. Three cubic inches were formed, of the quality of $\frac{86}{100}$ phoſoxygen and $\frac{14}{100}$ azote. After this, all the gas formed, contained from $\frac{76}{100}$ to $\frac{86}{100}$ of phoſoxygen. The other gaſes liberated with the phoſoxygen, appeared to be nitrogen and carbonic acid: at the latter part of the experiment, there was ſome indication of the preſence of hydrogen. The whole quantity of gas produced from 214 cubic inches of water, in thirty-ſix hours of ſunſhine, was 46 cubic inches of the mean quality of $\frac{80}{100}$ phoſoxygen, and $\frac{20}{100}$ of a gas or gaſes indiminiſhable by nitrous oxyd.

I have made a number of experiments on the decompoſition of water and carbonic acid by the marine cryptogamiæ, &c. the particulars of which it is unneceſſary to mention here. I ſhall give an account of them in an eſſay on the generation of phoſoxygen. A few obſervations made in the courſe of theſe experiments, will I think, afford additional proofs of the theory delivered in this eſſay; and therefore I ſhall relate them.

Firſt, An increaſed or diminiſhed temperature produced no ſenſible difference in the production of gas.

Secondly, A very ſtrong artificial light acted ſimilarly in the production of gas, to the ſolar light.

Thirdly, The capillary, dark, coloured, and opaque confervœ, generated more and better gas, than the white or pellucid. The confervæ generated more and better gas than the ulvæ, and the ulvæ more than the fuci.

Carbonic acid is not decompoſable by light alone. No alteration is effected in it, though expoſed for any length of time to the ſolar rays; nor is it decompoſable by vegetables; which, as is evident from their analyſis, attract carbon in large quantities. Theſe facts I have proved by experiments of which an account will be given in an eſſay on the generation of phoſoxygen.

Carbonic acid is decompoſable by the force of two attractions; that of any vegetable baſe, or carbon attractor for its carbon; and that of light, for its oxygen, as the following experiment will prove.

EXPERIMENT XIV.

A plant of Arenaria Tenuifolia planted in a pot filled with very dry earth, was inserted in carbonic acid, under mercury. The apparatus was expoſed to the ſolar light, for four days ſucceſſively, in the month of July. By this time the mercury had aſcended conſiderably. The gas in the veſſel was now meaſured. There was a deficiency of one ſixth of the whole quantity. After the carbonic acid was taken up by potaſh, the remaining quantity equal to one ſeventh of the whole, was phoſoxygen almoſt pure. From this experiment, of which a further detail will be given in an essay on the generation of phoſoxygen, it is evident that carbonic acid is decompoſed by two attractions; that of the vegetable for carbon, and of light for oxygen: the carbon combines with the plant, and the light and oxygen combined are liberated in the form of phoſoxygen.

Thus we have direct ſynthetical as well as analytical proofs of the compoſition of phoſoxygen. It has been demonſtrated then, *that phoſoxygen is light combined with oxygen.*

I have heretofore poſſeſſed no balance ſufficiently accurate to determine exactly the deficiency of weight from the light liberated in different combuſtive proceſſes. It is probable that light is liberated in an imperceptible form in ſome combuſtions unaccompanied with great generation of repulſive motion. In theſe proceſſes we can determine its liberation only from the deficiency of weight perceived.

It would probably throw much light upon

the properties of the different oxyds and acids to determine the quantity of light entering into their compoſition. The active properties of this ſubſtance may in a great meaſure influence the effects produced by the bodies into whoſe compoſition it enters on the organs of ſenſe.

If it were poſſible to determine with accuracy the deficiency of weight in combuſtion ariſing from the liberated light we might be able to diſcover the quantities entering into the compoſition of the acids.

Independant of the great uſe of phoſoxygen, as the pabulum vitæ of organic beings, it is of the greateſt importance to man ſimply conſidered as the ſupplier of light and heat by combuſtion. On the decompoſition of phoſoxygen by combuſtible bodies the greater portion of the comforts and luxuries of life depends. Without combuſtion man might have wandered for ever barbarous and uncivilized in his native deſarts. By the help of combuſtion the artiſt and manufacturor fabricate the tools, by which we erect cities, ſubdue and cultivate the earth, and directly derive our ſupport. Aſſiſted by combuſtion Commerce erects the ſtately veſſel, ſubjugates the ocean, ſhowers plenty over every nation, and connects mankind together. By the arts, dependant on combuſtion, ſcience and philoſophy no longer confined to thinking individuals, exiſt in characters. The preſs has made them immortal, and will ever continue to extend their beneficial influence. And laſtly, aided by combuſtion, the ſage devotes to philoſophy the ſolitary hours of midnight; purſuing thoſe combinations of ideas, which producing inventions improve and ameliorate the condition of man.

I am inclined to believe from ſome circumſtances, obſerved in the courſe of theſe experiments; that light and oxygen combine in different proportions. This opinion at preſent I am not able to demonſtrate experimentally; but I think it is countenanced by a number of facts. The phænomena obſerved in the higher regions of the atmoſphere render this ſuppoſition extremely probable. Reſpiration is painful on the tops of high mountains, and an inflammatory ſtate of the ſyſtem is induced; combuſtion is carried on with greater facility and at a lower temperature than on the plain, as M. Saſſure found that carbon caught fire ſooner and burnt quicker on the top of the Alps than on the plain beneath. Fiery meteors appear at an amazing height in the atmoſphere, much beyond that at which the ſolar rays are refracted:* and theſe meteors moſt probably ariſe from the formation of water.—We cannot account for theſe phænomena on any common principles. Phoſoxygen and Nitrogen are intimately combined here below; and they expand, when acted on by an increaſed repulſive motion in the ſame ratio. Now as the volumes of elaſtic fluids are in the inverſe ratio of their compreſſing weight; if phoſoxygen and nitrogen be ſuppoſed to compoſe the whole of our atmoſphere, then muſt the quantity of phoſoxygen in the atmoſphere decreaſe in proportion as the height increaſes.

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* One of these was computed by Dr. Halley to be above ninety miles high.

On this ſuppoſition the phænomena are totally inexplicable. For a deoxygenated*atmoſphere, instead of generating an inflammatory ſtate of the ſyſtem, tends to diminish it ; combuſtion is carried on with eaſe and rapidity in proportion as the quantity of phoſoxygen is greater ; and if the atmoſphere at 90 miles high be ſuppoſed to be compoſed of eminently rarified air, it is almoſt impoſſible that combuſtion could be carried on there. Theſe difficulties can be got over with eaſe on the ſuppoſition, that light and oxygen combine in different proportions. Light, continually acting upon the phoſoxygen of the atmoſphere, may combine with portions of it, and form a luminated phoſoxygen ; which muſt neceſſarily be

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* Nor will the ſubſtraction of preſſure from the veſſels account for this inflammatory ſtate of the ſyſtem, as action and reaction are equal.

of leſs ſpecific gravity, and eaſier of decompoſition than phoſoxygen; and this gas, from its ſmall ſpecific gravity, and probably ſtill farther combinations with light, may extend to an amazing diſtance from our planet.

The higher regions of the atmoſphere being ſuppoſed to be filled with this gas, combuſtion muſt take place on the tops of mountains at a lower temperature than on the plain, and with a greater liberation of light; becauſe the phoſoxygen there is combined with a greater proportion of light. Reſpiration muſt become painful, and an inflammatory ſtate of the ſyſtem be induced; becauſe the blood becomes ſuperſaturated with light; which as will be proved in the theory of reſpiration, is probably in a peculiar manner one cauſe of inflammation. The rays of light are not refracted in the atmoſphere above 45 miles high, becauſe beyond that the atmoſphere is amazingly rare, being compoſed of phoſoxygen highly luminated. Hydrogen aſcends in the atmoſphere, till it comes in contact with highly luminated phoſoxygen probably of the ſame ſpecific gravity : the oxygen looſely adhering to the light is attracted from it by the hydrogen at the common temperature of the atmoſphere to form water, whilſt the light is liberated, and hence the phœnomena of fiery meteors at a very great height.—I have invented experiments for the inveſtigation of this opinion ; and I hope to be ſoon poſſeſſed of the means for their execution.

From the great quantity of light liberated in many combuſtive proceſſes, particularly thoſe in which the phoſphoric, ſulphuric and carbonic acids and water are formed; we may fairly conclude that phoſoxygen is wholly decompoſed in theſe proceſſes. The light liberated and the oxygen attracted by the baſe ; yet there are others, as will be by and by proved, in which there is only a partial decompoſition of phoſoxygen. In theſe only a portion of light is liberated, whilſt the other part united to the oxygen combines with the attracting baſe. And phoſoxygen (oxygen and light) often combines with bodies, without decompoſition.

Theſe ſubſtances, which are now to be treated of, are combinations of light, oxygen and baſes.*

We have been obliged to form a new nomenclature for the combinations of phoſoxygen; neither that of the phlogistians, or of the caloriſts, would expreſs their compoſition with accuracy. On the modern principles of chemical nomenclature all compound ſubſtances ſhould be diſtinguiſhed by names characteriſtic of the ſubſtances forming the compound. We have endeavoured to adhere to this plan. All the combinations of phoſoxygen that have acid properties, are denoted by the names of phoſacids; thoſe which have no acid properties, are called phoſoxyds. By theſe terms the compounds of light, oxygen, and baſes, will be ſufficiently diſtinguiſhed from the combinations of oxygen and baſes which are ſimply called oxyds and acids.

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* From the experiments related by chemical writers on the metallic oxyds, one can draw no certain concluſions concerning the light liberated in oxydation. The peculiar properties of theſe bodies and their uſes, have been more attended to than the proceſs of oxydation.—I intend as ſoon as an opportunity offers to engage in a ſet of experiments on oxydation.

The terminations *ous* and *ic*, after the principles of the French nomenclators, will ſignify the different quantities of phoſoxygen entering into the compoſition of the phoſoxyds and phoſacids. The names of the acidifiable and oxydable baſes are nearly the ſame as in the French nomenclature. We have ſubſtituted nitrogen for azote, after Chaptal, Pearſon, and ſome other chemiſts.

COMBINATIONS of PHOSOXYGEN.

Subſtances that combine with Phoſoxygen.	*Combinations of PHOSOXYGEN and Subſtances.*	
New and Old Names.	*New Names.*	*Old Names.*
Nitrogen	1 Nitrous Phoſoxyd 2 Nitric Phoſoxyd 3 Nitrous Phoſacid 4 Nitric Phoſacid	1 Gaſeous Oxyd of Azote 2 Nitrous Gaz 3 Nitrous Acid 4 Nitric Acid
Muriatic Acid	Muriatic Phoſacid	Oxygenated Muriatic Acid
Platina -	Platinic Phoſoxyd	Oxyd of Platina
Gold -	Auric Phoſoxyd	Oxyd of Gold
Silver -	Argentic Phoſoxyd	Oxyd of Silver
Mercury -	Mercuric Phoſoxyd	Red Oxyd of Mercury
Lead -	Plumbic Phoſoxyd	Red Oxyd of Lead
Tungſten -	Tunſtic Phoſoxyd	Yellow Oxyd of Tungſten
Manganeſe	Manganeſic Phoſoxyd	Oxyd of Manganeſe
Chrome -	Chromic Phoſacid	Acid of Chromie*
Cobalt -	Cobaltic Phoſoxyd	Roſe colored Oxyd of Cobalt
Other Metals	Quere	Quere

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* This Acid has been lately diſcovered by Vauquelin in the red lead of Siberia.

COMBINATIONS of the NITROUS and NITRIC PHOSACIDS with SUBSTANCES.

Subſtances that combine with the Nitrous and Nitric Phosacids.	*Combinations of the NITROUS and NITRIC PHOSACIDS with Subſtances.*			
New and Old Names.	*New Names.*		*Old Names.*	
Barytes -	Phoſnitrite Phoſnitrate	of Barytes	Nitrite Nitrate	of Barytes
Strontian -	Phoſnitrite Phoſnitrate	of Strontian	Nitrite Nitrate	of Strontian
Potaſh -	Phoſnitrite Phoſnitrate	of Potaſh	Nitrite Nitrate	of Potaſha
Soda -	Phoſnitrite Phoſnitrate	of Soda	Nitrite Nitrate	of Soda
Lime -	Phoſnitrite Phoſnitrate	of Lime	Nitrite Nitrate	of Lime
Magneſia -	Phoſnitrite Phoſnitrate	of Magneſia	Nitrite Nitrate	of Magneſia
Ammonia -	Phoſnitrite Phoſnitrate	of Ammonia	Nitrite Nitrate	ofAmmonia
Argilla -	Phoſnitrite Phoſnitrate	of Argilla	Nitrite Nitrate	of Argilla
Metallic Oxyds and Phoſoxyds	Phoſnitrites Phoſnitrates	of Metals	Nitrite Nitrate	of Metals

COMBINATIONS *of* PHOSOXYGEN.

COMBINATIONS *of* NITROGEN *with* PHOSOXYGEN.

Nitrogen forms seventy-two hundred parts of the air of our atmoſphere. With regard to the preſent ſtate of our knowledge it muſt be conſidered as an undecompounded ſubſtance.

It enters into combination with a number of bodies. In organic compounds it is found in conſiderable quantities; and appears to act an important part in the phœnomena of life. Phoſoxygen and nitrogen combine in different proportions, and forms ſubſtances poſſeſſing ſpecifically different properties.

When phoſoxygen and nitrogen are made to combine by the action of the electric ſpark, it appears that no light is liberated in the proceſs. In this experiment phoſnitric acid is formed. And as phoſoxygen is compounded of light and oxygen, and nitrogen is a ſimple ſubſtance, it is evident from this experiment that phoſnitric acid is a compound of light, oxygen and nitrogen. The proof from analytical experiment is even more concluſive, and will account for a phœnomenon which the other theories were inadequate to explain.

EXPERIMENT XV.

Phoſnitrate of potaſh mingled with half its weight of carbon was fired by a burning glaſs in the exhauſted receiver. The conflagration took place and a conſiderable quantity of light was liberated in the repulſive projectile ſtate. The gaſeous products were examined and proved to be nitrogen and carbonic acid. The fixed ſubſtance remaining after combuſtion was potaſh mingled with carbon. The quantities of the products were not accurately aſcertained, as the end of this experiment was ſimply to determine their nature.

Nitric phoſacid then is compounded of light, oxygen and nitrogen. From the experiments of Lavoiſier we may conclude, that one hundred parts of it contain ſeventy-nine and a half of phoſoxygen, and twenty and a half of nitrogen. When carbon heated to a certain degree is placed in contact with phoſnitrate of potaſh it attracts the oxygen of the nitric phoſacid, and combines with it to form carbonic acid: the light and nitrogen having no combining attraction for each other, or for potaſh, are liberated, one in the repulſive projectile and the other in the gaſeous form. The great increaſe of repulſive motion is produced from the rapid divellent and combining chemical motions generated in the proceſs. The detonation is occaſioned by the undulatory motion generated in the circumambient atmoſphere by the rapid diſlodgment of a body of air equal in bulk to the elaſtic fluids generated in the proceſs.*

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* Lavoiſier, and the Caloriſts, ſuppoſe this detonation to be occaſioned in a great meaſure by the liberation of Caloric. They aſſert that when oxygen gas and azotic gas combine to form nitric acid, they retain in their compoſition a

The nitric phoſacid is decompoſable by increaſed repulſive motion alone, into nitrogen and phoſoxygen.

It combines with water, with the alkalies, the alkaline earths and metallic oxyds, forming compounds formerly called nitrates; but which to expreſs their compoſition more accurately, we have called phoſnitrates.

When nitric phoſacid is expoſed to light, it loſes a portion of its oxygen and light, and becomes nitrous phoſacid, as was before ob-

great quantity of the caloric which rendered them gaſeous. This caloric they ſay is liberated in the decompoſition of Nitrate of Potaſh, and hence, the increaſe of temperature, detonation, &c. This hypotheſis is one of the moſt abſurd advanced by the Caloriſts—On their theory of caloric, it is evident, that when the temperatures of bodies are increaſed in chemical proceſſes, their capacities muſt be diminiſhed; and therefore, the capacities of carbonic acid, azote and potaſh, muſt be much leſs than thoſe of carbon and nitrate of potaſh, than which nothing is more falſe: for I have found by experiment, that the united capacitiy of nitrate of potaſh and carbon, is much leſs than that of carbonic acid or azote, and independent of this, they have totally neglected the liberated light.

ſerved. One hundred parts of this acid appears to contain about ſeventy-four parts phoſoxygen, and twenty-ſix nitrogen. Like the nitric phoſacid it is decompoſable by increaſed repulſive motion into phoſoxygen and nitrogen; and when combined with baſes, by certain heated oxygen attractors, into light, oxygen and nitrogen. The nitrous phoſacid combines with water, with the alkalies, alkaline earths, metallic oxyds, &c. With the alkalies, alkaline earths, and metallic oxyds, it forms compounds which poſſeſs like the phoſnitrates, the property of detonating, i. e. of being rapidly decompoſed by heated oxygen attractors; to theſe ſubſtances we have given the names of phoſnitrites.

When a conſiderable portion of oxygen and light is ſubtracted from the nitric phoſacid by metallic ſubſtances, the gas liberated during the proceſs is nitric phoſoxyd.

This ſubſtance combines in ſmall proportions with water, is a permanent gas at the common temperature of the atmoſphere, and appears to contain about ſixty-eight per cent phoſoxygen, and thirty-two nitrogen. Phoſphorus decompoſes it by attracting the oxygen to form phoſphoric acid, whilſt the light and nitrogen are liberated.

Dr. Prieſtley diſcovered, that when nitric phoſoxyd (nitrous air) is expoſed to the action of moiſtened iron filings for a certain time, a diminution of its volume takes place, and a gas is formed, poſſeſſing peculiar properties, capable of ſupporting the flame of a candle better than atmoſpheric air; but at the ſame time totally unfit for the reſpiration of animals. To this gas he gave the name of dephlogiſticated nitrous air; but we have called it from its compoſition, nitrous phoſoxyd. It appears to contain leſs oxygen, and a larger proportional quantity of light than nitric phoſoxyd, as will appear from the following obſervations:

OBSERVATION *g*.

When nitric phoſoxyd is expoſed to the action of heated iron for a certain time, the iron becomes oxydated, and nitrous phoſoxyd is formed. No light is liberated during the proceſs. The oxyd of iron formed in this manner, is in every reſpect ſimilar to that formed by direct decompoſition of phoſoxygen.

OBSERVATION *h*.

When the repulſive motion of phoſnitrate of ammonia is increaſed to a certain degree, a new arrangement of its principles take place. Water and nitrous phoſoxyd are formed, and a portion of azote is liberated. No luminous appearance is perceived during this proceſs. Now as nitric phoſoxyd and nitric phoſacid are compounded of light, oxygen, and nitrogen, and according to the foregoing obſervations, no light is liberated during the formation of nitrous phoſoxyd, it is evident, that it muſt be compoſed of nitrogen, light, and a ſmaller portion of oxygen. The Dutch chemiſts have concluded from experiments on its decompoſition, that one hundred parts of it contain thirty-ſeven and half oxygen, and ſixty-two and half nitrogen. The light entering into its compoſition, has never been attended to by any chemiſt. This gas combines in very ſmall proportions with water, and appears to poſſeſs no acid properties. It is decompoſable by hydrogen and by certain combinations of hydrogen and carbon. Sulphur, phoſphorus and carbon, appear incapable of attracting oxygen from it at any common temperature. I have found by experiment, that a candle burns in this gas with a flame larger and more brilliant than in a gas compoſed of a mixture of thirty-eight parts phoſoxygen, and ſixty-two nitrogen; which alone would prove that it contained a larger proportional quantity of light, than any of the other combinations of nitrogen and phoſoxygen. I have made ſome other experiments on the compoſition of this gas, and ſome on the effects produced by it on animals, which will be detailed in a diſtinct eſſay.

It is extremely probable that the air of our atmoſphere is a chemical combination of phoſoxygen and nitrogen. A mixture of twenty-eight parts of phoſoxygen, and ſeventy-two of nitrogen, is not exactly ſimilar to the air of our atmoſphere. We want however experiments to determine the truth or falſhood of this ſuppoſition. If the fact was proved, this gas might be called phoſoxyd of nitrogen.

COMBINATIONS of the MURIATIC PHOSACID (OXYGENATED MURIATIC ACID.)

Subſtances that combine with Muriatic Phoſacid.	*Combinations of the* MURIATIC PHOSACID *with Subſtances.*	
New and Old Names.	*New Names.*	*Old Names.*
Barytes -	Phoſmuriate of Barytes	Oxygenated Muriate of Barytes
Strontian -	Phoſmuriate of Strontian	Oxygenated Muriate of Strontian
Potash -	Phoſmuriate of Potaſh	Oxygenated Muriate of Potaſh.
Soda -	Phoſmuriate of Soda	Oxygenated Muriate of Soda.
Ammonia -	Phoſmuriate of Ammonia	Oxygenated Muriate of Ammonia. *
Lime -	Phoſmuriate of Lime	Oxygenated Muriate of Lime.
Magneſia -	Phoſmuriate of Magneſia	Oxygenated Muriate of Magneſia.
Argilla -	Phoſmuriate of Argilla	Oxygenated Muriate of Argilla
Metallic Oxyds and Phoſoxyds.	Phoſmuriates of Metals.	Oxygenated Muriates of Metals.

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* Quere?

Of the MURIATIC PHOSACID.

The muriatic acid has been long diſcovered. Analogy would induce us to ſuppoſe that it is a compound of oxygen with ſome acidifiable baſe; but we are at preſent poſſeſſed of no facts ſufficient to prove its compoſition. We have attempted to decompoſe this acid by paſſing phoſphoric vapor through muriate of lime ſtrongly heated; but no phoſphoric acid was formed, and the muriate of lime remained unaltered. The muriatic acid combines with phoſoxygen, and forms an acid poſſeſſing peculiar properties. To this acid diſcovered by Scheele, the French nomenclators have given the name of oxygenated muriatic acid, on the ſuppoſition that it was muriatic acid combined with oxygen. We have called it muriatic phoſacid, to expreſs the combination of Light, Oxygen, and Muriatic Acid. The following experiment will prove analytically that the muriatic phoſacid is a compound of light, oxygen, and muriatic acid.

EXPERIMENT XVI.

Phoſmuriate of potaſh was mingled with twice its weight of carbon, and fired by a burning glaſs in the exhauſted receiver. The detonation took place with great increaſe of temperature in the ſurrounding bodies. A great quantity of brilliant repulſive projectile light was liberated. The gaseous products, on examination, proved to be carbonic acid and muriatic acid, the fixed ſubſtances remaining after combuſtion, were carbon, potaſh, and a ſmall quantity of muriate of potaſh.

From this experiment it is evident that the muriatic phoſacid is compounded of light, oxygen, and muriatic acid. Phoſmuriate of potaſh is compounded of phoſmuriatic acid and potaſh. When carbon is placed in contact with this ſubſtance, and heated, it attracts the oxygen of the muriatic phoſacid ſtronger than it is attracted by the light and muriatic acid, and combines with it to form carbonic acid. The light and muriatic acid having no affinity for each other, are liberated. The great increaſe of repulſive motion generated in this proceſs ariſes from the rapid motions of the combining and liberated bodies.

The compoſition of the muriatic phoſacid may be proved by ſyntheſis, as well as analyſis; for muriatic acid is never phoſoxygenated; but by combining with phoſoxygen, or by attracting it from ſome of its combinations, as will be better underſtood hereafter.

The muriatic phoſacid is decompoſable by light, as was proved by obſervation *b*. It is likewiſe decompoſable by phoſphorus, ſulphur, carbon, and metallic ſubſtances, when their temperatures are ſlightly increaſed by friction or percuſſion.*

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* To account for theſe detonations, the French chemiſts were obliged (as in the caſe of the nitric phoſacid) to ſuppoſe an immenſe quantity of calcric in the compoſition of this acid, which is directly contradictory to Black's doctrine of capacity.

The muriatic phoſacid combines with potaſh, ſoda, the alkaline earths, metallic oxyds, phoſoxyds, &c. The phoſmuriate of potaſh appears to contain in its compoſition a ſtill larger quantity of oxygen and light than the muriatic phoſacid; for during the combination of the muriatic phoſacid with potaſh, a certain quantity of muriatic acid is found in combination with a portion of the potaſh. And when ſulphuric acid is poured on phoſmuriate of potaſh, light, phoſoxygen, and phoſmuriatic acid gas are liberated.* The phoſmuriate of ſoda poſſeſſes ſimilar properties to the phoſmuriate of potaſh, with greater ſolubility in water. The other combinations of the phoſmuriatic acid have not heretofore been much attended to. Berthollet found that it was incapable of combination with ammonia at the common temperature; when mingled with this ſubſtance, a double decompoſition takes place, and water, muriatic acid, and nitrogen, are the products. We have ſucceeded in combining this acid with ſtrontian.*

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* We thought it probable, from the phænomenon accompanying the decompoſition of the phoſmuriate of ſtrontian, about to be deſcribed, that the ſame effects would be produced when the phoſmuriate of potaſh was uſed. Sulphuric acid, of the ſpecific gravity of 2,25, was poured on a few grains of phoſmuriate of potaſh: vivid white light was inſtantly liberated, and phoſoxygen and phoſmuriatic acid gas given out with great rapidity.

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* I effected this combination by paſſing muriatic phoſacid gas through a ſaturated ſolution of ſtrontian lime heated above 200°. The ſtrontian lime was obtained by my friend Mr. Clayfield, from the ſulphate of ſtrontian diſcovered by him near Briſtol. We firſt attempted the combination by paſſing muriatic phoſacid gas through ſtrontian lime water at the temperature of 30°,—40°; but without ſucceſs. Mr. Clayfield propoſed to try the effect of cold. The temperature of the ſolution was lowered by ſnow and ſalt to 10°, and the gas paſſed through; but no conſiderable combination was effected. We then diſſolved as much earth as poſſible in boiling water, and paſſed the gas through the ſaturated ſolution. The combination immediately took place. The ſolution of phoſmuriate of ſtrontian was of a duſky orange color. We had ſome difficulty in obtaining the chryſtals of this ſalt, from its extreme ſolubility. By great evaporation and cooling, it gave fine needle formed chryſtals. Theſe chryſtals ſlightly detonated with phoſphorus and charcoal. Alcohol holding them in ſolution, burnt with a roſe colored flame. When ſulphuric acid was poured into a ſolution of this ſalt in water, with a deſign to prove its compoſition by ſyntheſis, a beautiful and unexpected phænomenon took place. The room was accidentally darkened at the moment that this experiment

The phoſmuriate of ſtrontian poſſeſſes extreme ſolubility; its chryſtallization is ſimilar to that of the muriate of ſtrontian, and like that ſalt, when diſſolved in alcohol, it communicates to it the property of burning with a roſe colored flame. It detonates ſlightly with carbon and phoſphorus. During the decompoſition of this ſalt by the ſulphuric acid, a beautiful phænomenon takes place, the liberation of variouſly colored light.

It is not improbable that by attending to the combinations of the muriatic phoſacid, we may

was made, ſo that we were enabled to perceive a vivid luminous appearance whilſt muriatic phoſacid gas was liberated with great increaſe of temperature. We repeated the experiment two or three times with the ſame reſult, except that the light was differently colored. When ſulphuric acid of the ſpecific gravity of 2,25 was poured on the dry ſalt, no light was liberated, and the decompoſition went on very ſlowly; on the addition of water, the effects before deſcribed again took place. This experiment, independent of its beauty, is extremely pleaſing, as affording an inſtance of a true combuſtion, that is, the production of light and heat, by the mixture of two incombuſtible bodies.

diſcover ſome cheap ſubſtitute for the phoſnitrate of potaſh. From the preſent conſumption of this ſubſtance in the murder of mankind, the nitric phoſacid is an extremely expenſive article. The great importance of this acid in chemiſtry and the arts, renders the cheaper acquiſition of it a great deſideratum.

COMBINATIONS *of* LIGHT, OXYGEN, *and* METALLIC SUBSTANCES.

Since the diſcovery of oxydation by the great Lavoiſier, chemical philoſophers in general have conſidered the proceſs as the ſimple decompoſition of oxygen gas (oxygen and caloric); the combination of oxygen with the oxydable baſe, and the liberation of caloric. All metallic oxydations have been conceived to be ſimilar, and differing only in the rapidity of the decompoſition of oxygen gas. As light has been little attended to, even among the chemical principles of the moſt celebrated pneumatiſts, we are not ſurpriſed to find that it has been neglected in the proceſs of oxydation. The combinations of light, oxygen, and metallic bodies, have not been heretofore diſtinguiſhed from the ſimple combinations of oxygen with theſe ſubſtances, though from the differences of their properties alone they might well be conſidered as a diſtinct claſs of ſubſtances. To theſe bodies we have given the name of phoſoxyds, to diſtinguiſh them from the ſimple combinations of oxygen, and to expreſs their compoſition with accuracy.

PLATINIC PHOSOXYD.

Platina appears incapable of directly combining with light and oxygen at any known temperature. This combination is effected by the decompoſition of the muriatic phoſacid, or the murionitric phoſacid. In the decompoſition of the muriatic phoſacid by platina, the platina combines with the light and oxygen, ſo as to convert the acid into muriatic acid. The properties of this phoſoxyd are but little known, and I have never had an opportunity of examining them.

AURIC PHOSOXYD.

Gold, which is incapable of decompoſing, or of directly combining with, phoſoxygen at any known temperature, poſſeſſes the power of attracting it from ſome of its combinations. Gold becomes phoſoxydated by attracting light and oxygen from the muriatic phoſacid or the murionitric phoſacid; it is extremely probable, from the properties of this phoſoxyd, that it contains a larger proportion of oxygen, and a ſmaller proportion of light, than the other phoſoxyds. Expoſed to light, it gives out phoſoxygen and the metal is revivified. It combines with the acids, and with ammonia; with ammonia it forms the compound called fulminating gold, but which might be called with greater propriety, auric phoſoxyd of ammonia. From the decompoſition of this ſubſtance alone, we might prove that light was one of the conſtituent parts of auric phoſoxyd. When the temperature of auric phoſoxyd of ammonia is increaſed a little, it is decompoſed with a great exploſion, and the products are light, nitrogen, and water. In this process the hydrogen of the ammonia combines with the oxygen of the auric phoſoxyd to form water, and the nitrogen of the ammonia, and the light of the phoſoxyd, having no attraction for each other or for water, are liberated.

ARGENTIC PHOSOXYD.

Silver, like gold and platina, is incapable of combining with phoſoxygen directly, at any known temperature. Silver appears capable of combining with oxygen, as well as phoſoxygen, and conſequently it decompoſes both the acids and phoſacids. It attracts oxygen and light from the nitric phoſacid with great rapidity, and becomes phoſoxydated, whilſt the remaining light and oxygen of the acid fly off with the nitrogen in the form of nitric phoſoxyd. The argentic phoſoxyd combines with the acids, and with ammonia. Its combination with ammonia affords one of the moſt aſtoniſhing phænomena in chemiſtry. To form this ſubſtance, the phoſoxyd of ſilver muſt be precipitated from its ſolution in nitric phoſacid by lime-water. This precipitate after expoſure to light for ſome hours, muſt be ſtirred in a ſolution of ammonia. When this ſolution is evaporated, the chryſtaliſed ſubſtance remaining is the fulminating ſilver, or argentic phoſoxyd of ammonia. The ſlighteſt poſſible change of temperature by friction, percuſſion, or any other means, cauſes this ſubſtance to explode with an aſtoniſhing detonation, and the liberation of light. The products of this detonation have never been examined. Light we know is one of them, the others are probably water and nitrogen: for the ſubſtance is a compound of light, oxygen, ſilver, nitrogen, and hydrogen. The ſlighteſt change of temperature diſpoſes the light to fly off, and the hydrogen and oxygen to combine to form water. The great increaſe of temperature, and the detonation inexplicable on the former theories, are accounted for with the greateſt eaſe on the theory of repulſive motion, without any abſurd or unnatural ſuppoſitions.

MERCURIC PHOSOXYD.

Mercury combines directly with phoſoxygen, as appears from the following obſervations.

OBSERVATION *i.*

When mercury is placed in contact with phoſoxygen, and its temperature raiſed nearly to its boiling point, the phoſoxygen and mercury are gradually converted into a red ſubſtance poſſeſſing properties eſſentially different from mercury or phoſoxygen. No light is liberated in this proceſs. The phoſoxyd of mercury is conſequently a combination of light, oxygen, and mercury. This ſubſtance is likewiſe formed by the decompoſition of the nitric phoſacid. Its fine red color, like that of moſt other ſubſtances, appears to depend on the light entering into its compoſition. It combines with the acids, and with ammonia. With ammonia it forms a fulminating compound, the mercuric phoſoxyd of ammonia.

The mercuric phoſoxyd is decompoſable by light and increaſed repulſive motion, into phoſoxygen and mercury.

PLUMBIC PHOSOXYD.

The plumbic phoſoxyd (red oxyd of lead) evidently contains oxygen and light; for when the white oxyd of lead is heated in contact with phoſoxygen, it becomes red and more ponderous, and the phoſoxygen is abſorbed without the liberation of light. When muriatic acid is diſtilled from the plumbic phoſoxyd, it becomes muriatic phoſacid. From whence we may conclude that the plumbic phoſoxyd is compounded of oxygen, light, and lead.

TUNGSTIC PHOSOXYD.

The tungſtic phoſoxyd (yellow oxyd of tungſten) appears to contain light in its compoſition. It gives out phoſoxygen, and becomes blue on expoſure to the ſolar light. It appears capable of combining with the alkalies, alkaline earths, &c.

MANGANESIC PHOSOXYD.

The manganeſic phoſoxyd (black oxyd of mangeneſe) evidently contains phoſoxygen; it affords it on the application of heat. When muriatic acid is diſtilled from it (the mangeneſic phoſoxyd,) it becomes muriatic phoſacid, that is, it ſubtracts a portion of light and oxygen from the manganeſic phoſoxyd.

CHROMIC PHOSACID.

This acid, juſt diſcovered by the ingenious Vauquelin, appears to contain light; its red color alone would induce us to believe this, and from the experiments of Vauquelin, it appears that it loſes its red color when expoſed to light, and moſt probably gives out a portion of light and oxygen.

COBALTIC PHOSOXYD.

The cobaltic phoſoxyd is formed by the decompoſition of the nitric phoſacid.

We have found that it gives out phoſoxygen when its repulſive motion is increaſed, from whence we conclude that it is a combination of light, oxygen, and cobalt.

The phoſoxyds poſſeſſed of the moſt ſtriking properties are, the auric, argentic, mercuric, and plumbic. The facility with which theſe bodies are decompoſed, is evidently dependant on the light entering into their compoſition. Indeed we have no ſingle inſtance of a chemical detonation independant of the preſence of light. The eaſe with which the equilibrium of the principles of the phoſoxyds of ammonia is deſtroyed, depends on the eaſe with which light takes its repulſive projectile form. We are in want of a ſet of accurate experiments on the proceſs of oxydation and phoſoxydation. It is probable that many metals, beſides thoſe we have mentioned, are capable of combining with phoſoxygen. The different colors and properties of the different oxyds of the ſame metal may probably depend on certain quantities of light and oxygen entering into their compoſition.

Such then are the inorganic combinations of light. From the diſcovery of them we are enabled to explain the phænomena of combuſtion, detonation, &c. They open to us an extenſive field for experimental inveſtigation. It is probable that we ſhall detect light in many other ſubſtances, in which it has not been hitherto ſuſpected. That extenſive claſs of ſubſtances that has been heretofore diſtinguiſhed by the common name of oxygen-attracting, muſt be divided into diſtinct claſſes, from the differences of the attractions of the ſubſtances compoſing it. Some of theſe ſubſtances, as phoſphorus, ſulphur, &c. are ſimple attractors of oxygen, that is, decompoſers of phoſoxygen. Others, as iron, zinc, copper, &c. under different circumſtances, appear to attract oxygen and portions of light with oxygen. Another claſs combines with phoſoxygen, or light and oxygen, without decompoſition, as nitrogen, muriatic acid, mercury, &c.

We perceive a correſpondence between the quantities of light and oxygen entering into the compoſition of bodies, and their colors. By attending to this circumſtance, and by determining the quantities of light liberated in oxydation, and comparing the properties of the oxyds, phoſoxyds, &c. we may make ſome diſcoveries in an important branch of corpuſcular philoſophy hitherto unknown.—The cauſes of the differences of the capabilities of bodies for receiving the communicated repulſive motion of light, that is, the cauſes of the differences of their colors.

RECAPITULATION.

This recapitulation is deſigned to preſent the theory founded upon the experiments deſcribed, in a ſhort view capable of being at once conſidered by the mind.

1ſt. Matter is ſuppoſed to be endowed with the active properties of repulſive motion and attraction. By the terms repulſive motion and attraction, we ſimply mean to expreſs the cauſes of certain effects which are uniformly and conſtantly produced. In denying the exiſtence of caloric, we do not aſſert that there does not exiſt a number of ſubſtances which are totally incogniſable by our ſenſes; but we conſider all matter as governed by the ſame laws, and as active properties muſt be aſcribed to ſome matter, and as we perceive the effects of them in that matter with which we are acquainted, we have a right to conclude that they belong to it. Nothing is more unphiloſophic than to imagine beings for the ſake of attributing to them active powers, when our ſenſations inform us of the exiſtence of bodies to which they belong.

2. The moſt ſubtile etherial fluids with which we are acquainted, are governed by the laws of attraction and repulſive motion. Amongſt theſe is light, which acts the moſt important part in the œconomy of the univerſe. This ſubſtance is ſubject to the common laws of matter, and requires no principles, but attraction and repulſive motion, to account for its appearances and changes.

3. It enters into combination with bodies. In the phoſphoreſcent bodies it exiſts in a ſtate of looſe combination.

In phoſoxygen it is intimately combined with oxygen.

4. From the decompoſition of phoſoxygen by bodies that attract oxygen, the phænomena of combuſtion are explained.

5. Phoſoxygen combines with ſubſtances without decompoſition.

Theſe ſubſtances are nitrogen, muriatic acid, and certain metals. On the combination of phoſoxygen with theſe bodies, the phænomena of detonation &c. depend.

LIGHT enters into the compoſition of living bodies. To underſtand theſe combinations is of infinite importance to man. On the exiſtence of this principle in organic compounds, perception, thought, and happineſs, appear to depend. We ſhall proceed to inveſtigate the theory of reſpiration.

THEORY *of* RESPIRATION.

The dependance of life on reſpiration has always rendered it an important ſubject of conſideration to phyſiologiſts; but till the diſcovery of pneumatic chemiſtry, no rational theory was advanced. It is foreign to our preſent deſign to conſider all the different theories that have been formed. The theory of reſpiration now generally received by phyſiologiſts, is that advanced by Godwyn, Girtanner, &c. Theſe philoſophers aſſume oxygen gas to be oxygen combined with caloric; and ſince it is found by experiment that the oxygen gas made uſe of in reſpiration is diminiſhed, and carbonic acid and water formed; it is aſſerted that oxygen gas is decompoſed in the lungs. It is ſaid, that one portion of the oxygen combines with the oxydable and acidifiable baſes in the venous blood, and particularly with the iron; and that from this oxydation ariſes the vermilion color of the arterial blood: another portion combines with the carbon of the blood and of the pulmonary mucus, to form the carbonic acid liberated in reſpiration; another portion combines with the hydrogen of the blood, to form the water liberated in reſpiration: the caloric combined with the oxygen partly combines with the blood, now increaſed in capacity, and is partly liberated, with the carbonic acid and aqueous gas. Without conſidering my experiments, or the theory of theſe papers, which directly overturn this hypotheſis, there are the following objections to it.

1ſt. Iron which is generally aſſumed to be the reddening principle of the blood, never decompoſes phoſoxygen at ſo low a temperature as 98°, the greateſt heat of the lungs, and phoſoxygen is never decompoſed by iron without rapid combuſtion, flame, and great heat.

2d. Oxygen gas is never decompoſed by carbon at ſo low a temperature as 98°, and is never decompoſed without combuſtion, &c.

3d. There is never a decompoſition of phoſoxygen by hydrogen at ſo low a temperature, and it is well known that this proceſs does not take place without flame.

This theory of reſpiration, then, is evidently falſe. It will appear from the following experiments and obſervations:

1ſt. That phoſoxygen (light and oxygen) is not decompoſed in the lungs.

2d. That phoſoxygen (light and oxygen) combines with the venous blood in the lungs.

3d. That carbonic acid and water are both liberated from the lungs during this proceſs, either by the increaſe of temperature, or from the ſuperior affinity of phoſoxygen for the venous blood.

EXPERIMENT XVII.

A phial containing twelve cubic inches and a half, was filled with very pure phoſoxygen.

The medial vein of a healthy man was opened, and the ſtream of blood directed into the phial. The mouth of the phial was immediately brought in contact with the arm, ſo as to entirely exclude all external air. The room was then darkened. As the blood flowed in, it changed from a dark red to a bright vermilion color. When the phial was half full it was cloſed, and plunged in mercury heated to 90°. After remaining in this ſituation for half an hour, it was examined. The blood had coagulated, and was of a bright vermilion; ſome drops of water were formed on the ſides of the phial. When the cork was drawn, about two cubic inches of mercury ruſhed into the phial, from whence I concluded that an abſorbtion of gas had taken place. The gas remaining in the phial was examined. It proved to be three cubic inches and one tenth of phoſoxygen, mingled with nine tenths of a cubic inch of carbonic acid.

During this experiment no light was liberated; it is conſequently reaſonable to ſuppoſe that there was no decompoſition of phoſoxygen;

and as a conſiderable diminution of phoſoxygen took place, and the blood acquired new properties, we may conclude that phoſoxygen is capable of combination with the venous blood. To prove this by analyſis as well as ſyntheſis, I made the following experiment.

EXPERIMENT XVIII.

A phial containing about twelve cubic inches, having a pneumatic apparatus affixed to it, was filled with arterial blood from the carotid artery of a calf. The phial was placed in a ſand bath of the temperature of 96°, and the heat gradually and ſlowly raiſed. In about ten minutes the temperature of the bath was 108°, and the blood began to coagulate. At this moment ſome globules of gas were perceived paſſing through the tube. Gas continued to paſs in very ſmall quantities for about half an hour, when the temperature of the ſand was about 200°; the blood had coagulated perfectly and was now almoſt black: about a cubic inch and eight tenths of gas were collected in the mercurial apparatus; of this one cubic inch and one tenth was carbonic acid, and the remaining ſeven tenths phoſoxygen.*

From this experiment it is evident that the arterial blood contains phoſoxygen, and we have proved before by ſyntheſis, that it is capable of combining with it directly. We are poſſeſſed of a nunber of experiments, which prove that phoſoxygen is conſumed in reſpiration. It has been likewiſe proved that gaſes can penetrate through moiſt membranes like thoſe of which the veſſels in the lungs are compoſed. We may therefore conclude that phoſoxygen combines with the venous blood of the ſyſtem in the pulmonary veſſels. As no light was liberated in Experiment XVII. it is evident that there cannot be even a partial decompoſition of phoſoxygen in reſpiration, and conſequently the carbonic acid and aqueous gas liberated cannot ariſe from the decompoſition of phoſoxygen by the carbon and hydrogen of the venous blood. It is then evident that they muſt be liberated from the venous blood. To prove this more clearly, I made the following experiment.

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* Great caution is requiſite in making this experiment. If the temperature is not gradually and ſlowly increaſed, the liberated gaſes are carbonic acid and hydrogen. The firſt time that I made this experiment, raiſing the temperature too quickly, I obtained only theſe products. At a high temperature, the phoſoxygen of the blood moſt probably combines with nitrogen, to form nitric phoſacid.

EXPERIMENT XIX.

I filled a ſmall ſheep's bladder with blood from the medial vein of an healthy woman. This blood never came in contact with any air during the experiment. The bladder was inſerted in a veſſel of water heated to 112°, and the gaſeous products received by a pneumatic apparatus. They were carbonic acid and aqueous gas.

Reſpiration, then, is a chemical proceſs, the combination of phoſoxygen with the venous blood in the lungs, and the liberation of carbonic acid and aqueous gas from it. From the combination and decompoſition, ariſes an increaſe of repulſive motion, which combined with that produced by the other chemical proceſſes taking place in the ſyſtem, and that generated by the reciprocal action of the ſolids and fluids, is the cauſe of animal heat ; a heat which the other ſyſtems have ſuppoſed to ariſe chiefly from the decompoſition of oxygen gas (oxygen and caloric).

Such then is the human reſpiration ; and we are certain not only from analogy, but from experiments, that the breathing proceſs of quadrupeds and birds is ſimilar. Phoſoxygen (oxygen and light) combines with the venous blood in their lungs.

As fiſhes exiſt in a different element ; and as it has been ſuppoſed that they decompoſe both water and oxygen gas,* I endeavoured to aſcertain by the following experiments the laws of their reſpiration.

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* Darwin's Zoonomia, Vol I. p. 472. Oxygenation of the Blood.

EXPERIMENT XX.

I expelled by long boiling the atmoſpheric air from 64 cubic inches of ſea water. This by means of mercury I entirely excluded from the contact of air. A ſmall mullet was put into it, which inſtantly appeared much convulſed, and died in a few minutes.

EXPERIMENT XXI.

A quantity of water was freed from atmoſpheric air by boiling. Two receivers, each of the capacity of 36 cubic inches, were filled with this water. Into one of theſe, two cubic inches of nitrogen were inſerted, into the other, two of phoſoxygen. By long and conſtant agitation, the gaſes were diſſolved by the water, which was excluded from the contact of air by mercury. Into each of the receivers two minnows were inſerted. Thoſe in the water holding nitrogen in ſolution, died in about four minutes;

thoſe in the water holding phoſoxygen in ſolution, appeared totally uninjured, and when examined after ſome hours, were ſtill alive and healthy.

EXPERIMENT XXII.

The ſame receivers which I uſed in my laſt experiment, were filled with diſtilled water, freed from atmoſpheric air by a ſecond boiling. Into each of theſe, three cubic inches of phoſoxygen were inſerted. The receivers were then agitated for ſome time, till the water in each of them had diſſolved an equal quantity of gas; they were then inverted in a trough of mercury, ſo as to exclude atmoſpheric air from them. Four minnows were then conveyed into one of them through mercury. The receivers were now ſuffered to remain untouched for ſix hours, when they were examined. The minnows were alive, and no gas remained in the top of the receiver in which they had reſpired. The gas in the top of the other receiver remained nearly the ſame as at the commencement of the experiment.

A quantity of lime-water was poured into each of theſe receivers; in that in which the fiſh had exiſted there was a very perceptible cloudineſs, occaſioned as I ſuppoſe by the formation of carbonate of lime; in the other there was no perceptible change.

From theſe experiments I conclude that the venous blood in the gills of fiſhes is phoſoxydated by the phoſoxygen held in ſolution by water; and that carbonic acid, and probably water, are given out as excrementitious by the venous blood in their gills. We have no reaſons for ſuppoſing that fiſh decompoſe water, as we cannot diſcover that any hydrogen is formed by them in reſpiration.*

Light and oxygen then, (phoſoxygen) are eſſential to life.

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* I have diſcovered by ſimilar experiments that the zoophyta are governed by ſimilar laws: that they, like fiſh, abſorb the phoſoxygen held in ſolution by water, as well as portions of nitrogen; and thus in their chemical attractions, as well as in their organic powers, ſeem to be the connecting links between vegetables and animals.

The perceptive and volitive powers depend for their continued exiſtence on the conſtant ſupply of a certain quantity of phoſoxydated blood to the nervous and muſcular ſyſtems.

Perception more immediately depends on the continued ſupply of a certain quantity of arterial blood to the brain. In the brain and nervous ſyſtem, ſome important change eſſential to life muſt be effected by it. As there is a neceſſity for a conſtant ſupply of phoſoxygen to ſupport the vital functions, there muſt be a conſtant expenditure of it in the performance of theſe functions. The medullary ſubſtance of the brain and nerves appears to poſſeſs the property of ſenſibility. This property is perfectly diſtinct from the irritabilty or contractile power of the muſcular fibre. The nerves depend for their ſenſibility on their connection with the brain. In the brain all the ſenſations conveyed by the different nerves center; and in the brain their correſpondent ideas are aſſociated together according to certain laws. The moment that the connection of a nerve with the brain is deſtroyed, it ceaſes to be ſenſible ; and thought and action ceaſe the moment that a ſupply of phoſoxydated blood is cut off from the brain.

May we not venture to reaſon on the important and conſtant change effected in the brain and nerves by the phoſoxydated blood? Is it not probable that the exiſtence of ſome fine etherial principle in the brain and nerves, is the immediate cauſe of ſenſible or perceptive action? If ſuch a fluid exiſts, it muſt be continually ſupplied by the arterial blood, and conſtantly expended in ſenſible action. We have proved the exiſtence of light in the arterial blood, and we have likewiſe proved its exiſtence in different ſtates. Is it then improbable to ſuppoſe that LIGHT is attracted or ſecreted from the blood by the brain in the form of an etherial fluid or gas, and perpetually conveyed by the brain to the nerves?

A number of philoſophers ſimply from the identity of the action of the electric fluid, and the nervous influence on the irritable fibre, have concluded them to be the ſame, that is, have concluded the nervous fluid to be the electric aura.

We have before ſuppoſed the electric fluid to be condenſed light. Thus we have another cogent reaſon for ſuppoſing that the nervous ſpirit is light in an etherial gaſeous form.

On this ſuppoſition, ſenſations and ideas will be motions of the nervous ether or light exciting the medullary ſubſtance of the nerves and brain into ſenſitive action. The capability of the nerves to be excited into ſenſitive action by the motions of the nervous ether, muſt depend upon a peculiar conſtitution or organization of them; and the nerves during ſenſitive action muſt ſuffer ſome change, ſome loſs of principles, and the equilibrium of their principles muſt be again ſupplied by the arterial blood.

The irritability of the muſcles, as well as the ſenſibility of the nerves, appears to depend on the continued ſupply of a certain quantity of arterial blood. Their irritability is not nearly ſo ſoon deſtroyed as the ſenſibility of the nerves. The fibre remains irritable for a conſiderable time after it is deprived of arterial blood. The muſcles are moſt probably phoſoxydated compounds, of which the numerous principles are in exact and delicate equilibrium; and it is likely that on this equilibrium their irritability depends. Any communicated impulſe capable of producing increaſe of repulſive motion ſufficient to occaſion a new arrangement of principles in any part of the fibre, will produce irritable action. The impulſes of certain external bodies, and the nervous motions, both appear equally capable of exciting the irritable fibre into action.

The chief principles * of the fibre appear to be nitrogen, hydrogen, carbon, oxygen, and

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* The inorganic compounds, which contain light, and many other principles, are moſt eaſily decompoſed by the ſlighteſt increaſe of repulſive motion, ſuch are the phoſoxyds of ammonia.

light.[†] The immediate cauſe of irritable action is probably the combination of the oxygen with the hydrogen and carbon, to form water and carbonic acid, and the liberation of azote and electric fluid. We are certain that water and carbonic acid are liberated during muſcular action, and probably azote, and light in the form of electric fluid.[*]

[†] When any conſiderable change takes place in the organic matter of the body, ſo as to deſtroy the powers of life, new chemical attractions and repulſive motions take place. The different principles of which the body is compoſed, form new combinations. In this proceſs, which is called putrefaction, the light of the ſyſtem in land animals in combination with oxygen and nitrogen, forms nitric phoſacid.

In fiſh, during whoſe putrefaction no nitric phoſacid is formed, it is liberated; and hence the reaſon for the luminous appearance of putrefying fiſh, an appearance, which Lavoiſier ſuppoſed to be occaſioned by phoſphoriſed hydrogen. I have found by experiment, that putrefying fiſh are equally luminous in water boiled to expel its air and phoſoxygen.

[*] The Torpedo, and ſome other animals, give out electric fluid during animal action. In man the quantity is probably however too ſmall and too ſlowly liberated to be aſcertainable. It would be worth while to try, by a very ſenſible electrometer, whether an inſulated muſcle when

Life, then, may be conſidered as a perpetual ſeries of peculiar corpuſcular changes; and the living body as the being in which theſe changes take place. Perceptions, ideas, pleaſures, and pains, are the effects of theſe changes. They are conſequently found to be continually varying. The laws of mind then probably, are not different from the laws of corpuſcular motion. Every change in our ſenſations muſt be accompanied with ſome correſpondent change in the organic matter of the body. Theſe changes an extenſive and philoſophic chemiſtry may enable us to eſtimate.

Thus eſſential then is LIGHT to perceptive exiſtence. All organic ſenſitive beings with

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ſtimulated into action, would not give marks of the liberation of electric fluid.—To aſcertain the gaſeous products liberated during muſcular action, would not be ſo difficult. An animal or the limb of an animal, might be ſtimulated into muſcular action for a conſiderable time under mercury, till its irritability was deſtroyed. Animals previouſly made to breathe phoſoxygen, might be employed for this purpoſe, as it appears from the experiments of a celebrated philoſopher, Dr. Beddoes, that they retain their irritability longer.

which we are acquainted appear totally unable to exiſt without phoſoxygen.

We may conſider the ſun and the fixed ſtars, the ſuns of other worlds, as immenſe reſervoirs of light deſtined by the great ORGANISER to diffuſe over the univerſe organiſation and animation. And thus will the laws of gravitation, as well as the chemical laws, be conſidered as ſubſervient to one grand end, PERCEPTION. Reaſoning thus, it will not appear impoſſible that one law alone may govern and act upon matter: an energy of mutation, impreſſed by the will of the Deity, a law which might be called the law of animation, tending to produce the greateſt poſſible ſum of perception, the greateſt poſſible ſum of happineſs.*

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* The analogy between attraction and gravitation, repulſion and projection, has been mentioned before. This analogy would induce us to refer them to the ſame cauſes. It may appear abſurd to ſuppoſe any analogy between theſe powers and the laws of life; Is not however perceptive

The farther we inveſtigate the phænomena of nature, the more we diſcover ſimplicity and unity of deſign.

An extenſive field for ſublime inveſtigation is open to us. The laws of perceptive life as yet are but partially known. Our ſenſations, ideas, pleaſures, and pains, depend upon cauſes now unknown to us.

We cannot entertain a doubt but that every change in our ſenſations and ideas muſt be accompanied with ſome correſpondent change in the organic matter of the body. Theſe changes experimental inveſtigation may enable us to determine. By diſcovering them we ſhould be informed of the laws of our exiſtence,

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action (which muſt uniformly be accompanied with ſome peculiar motion in the nervous ſyſtem) analogous to repulſion and projection? Is not the aſſociation of perceptive and irritative motions, a law analogous to attraction and gravitation?

and probably enabled in a great meaſure to deſtroy our pains and to increaſe our pleaſures.

Thus would chemiſtry, in its connection with the laws of life, become the moſt ſublime and important of all ſciences.

An ESSAY

On the GENERATION of PHOSOXYGEN,

Or OXYGEN GAS;

And on the Cauſes of the COLORS of

ORGANIC BEINGS.

An ESSAY

On the GENERATION of PHOSOXYGEN,

(OXYGEN GAS)

And on the Cauſes of the COLORS of

ORGANIC BEINGS.

A great quantity of phoſoxygen is continually attracted from the atmoſphere to ſupply the pabulum vitæ of perceptive beings. Another conſiderable quantity is decompoſed by combuſtion, and abſorbed in other chemical proceſſes which take place in our globe. Since the atmoſpheric conſtitution is uniformly ſimilar, we are led to enquire by what means a quantity of phoſoxygen equal to that conſumed by reſpiration and combuſtion is again ſupplied to the atmoſphere. This enquiry is no leſs important than curious, as our exiſtence itſelf depends on the equilibrium of gaſes in the atmoſphere.

We have no reaſon for ſuppoſing that phoſoxygen is generated or given out as excrementious by the locomotive organic beings which conſume ſuch immenſe quantities of it. During living action, water, carbonic acid, and nitrogen, are liberated from the animal, and probably electric ether and ſome other products.

We have no reaſons for ſuppoſing that light is capable of decompoſing either water or carbonic acid in their naſcent ſtate, by its attraction for oxygen. Conſequently the phoſoxygen of the atmoſphere can in no way be ſupplied by the locomotive perceptive beings.

The living action of another claſs of beings, namely vegetables, is the cauſe of the generation of the atmoſpheric phoſoxygen.

The purification of the atmoſphere by land vegetables was indeed diſcovered long before even its compoſition was known. We owe this important diſcovery to the immortal Prieſtley, who ſuppoſed that the renovation of the atmoſphere by vegetables was occaſioned by their power of abſorbing phlogiſton, an imagined product of combuſtion and reſpiration.

Dr. Ingenhouz diſcovered that vegetables gave out vital air when expoſed to the ſolar light in contact with water. This philoſopher attempted to prove by experiments, that the air thus generated aroſe from the decompoſition of water. Since his experiments and thoſe of M. Senebier have been publiſhed, we know of little that has been done in this part of chemiſtry: and as Lavoiſier's theory of the compoſition of oxygen gas (phoſoxygen) has been generally believed by chemical philoſophers, it has been ſuppoſed that light acted no other part in the decompoſition of water, than that of extricating oxygen by giving it caloric, whilſt the vegetable attracted hydrogen.

In my eſſay on Repulſive motion and Light, I attempted to prove that water was decompoſable by two attractions, that of a vegetable or organiſed hydrogen attractor for hydrogen, and that of light for oxygen. In that eſſay I likewiſe mentioned my diſcovery of the production of phoſoxygen by the various orders of the marine cryptogamia claſs of plants. I ſhall proceed to give a detail of the experiments I have made on land and ſea vegetables.

One might infer from the analyſis of vegetables that they attract hydrogen and carbon, as they are their moſt predominant principles. It will appear from the following ſynthetical experiment that they attract hydrogen.

EXPERIMENT I.

A ſmall plant of Minianet in a ſtate of healthy vegetation was inſerted in a ſmall pot under a jar of hydrogen containing 28 cubic inches, and confined by mercury. The height of the mercury in the jar was accurately marked at the commencement of the experiment. It was ſuffered to remain in the dark for fifty hours, and then examined. The mercury had aſcended a very little, and if one might judge from appearances, there was a diminution of about half a cubic inch. The jar was removed to a place expoſed to the light, and ſuffered to remain in this ſituation for three days of very fine weather. It was then examined: the plant appeared uninjured, and the edges of ſome of its leaves were tinged yellow; there was no perceptible diminution of the mercury, from whence I concluded that phoſoxygen had been generated. Solutions of ſulphure of potaſh, and of potaſh were introduced into the jar in ſeparate veſſels. A diminution took place. The remaining gas meaſured about twenty five and half cubic inches, and appeared to be pure hydrogen.

Since, then, two cubic inches and half of hydrogen diſappeared in this experiment, they were moſt probably abſorbed by the plant.

I have found by ſeveral experiments, that Minianet always grows very well in hydrogen when ſupplied with water. One of theſe plants lived in hydrogen for a week in the dark, ſupplied with water by a capillary tube. At the end of this time the leaves became variegated with yellow ſpots, and the whole of the plant bore a ſickly appearance. When theſe plants growing in hydrogen are deprived of water, they generally die in three or four days. In the experiment that I have juſt related, the earth of the pot in which the plant grew was moiſtened previous to its inſertion, and then covered with a tin-plate top, to prevent the water from evaporating. Some plants die in hydrogen very quickly; among theſe are Conium maculatum, Chironea centaurium, Digitalis purpurea.

In a mixture of hydrogen and carbonic acid, I found no plant to die when expoſed to light. From a number of experiments, I think I have every reaſon to conclude that plants directly combine with hydrogen; but as they owe their irritability, that is, their life, not to one principle alone, but to many, it is difficult to invent ſimple experiments to determine ſingly their attraction for each.

As vegetables attract hydrogen, and as it is one of their moſt predominant principles if it be conſidered as a ſimple ſubſtance, it muſt be obtained by them from the decompoſition of bodies into which it enters as a conſtituent part.

Vegetables are totally incapable of decompoſing water by their attraction for hydrogen. Some land plants give out carbonic acid, and ſmall portions of nitrogen during the night; but there is no ſingle inſtance of the production of phoſoxygen from plants during the night. To determine whether the water plants which ſtrongly attract hydrogen produce any gas in the abſence of light, I made the following experiment.

EXPERIMENT II.

Into a green glaſs globe containing one hundred and four cubic inches, a plant of nymphæa alba in a ſtate of healthy vegetation was inſerted. The globe was filled with water previouſly boiled to expel the atmoſpheric air, and inſerted horizontally in a veſſel, ſo as to be perfectly included from light. After remaining in the dark for four days it was examined. Two or three inconſiderable globules of gas were found in the top; which ſuffered no diminution when mingled with nitric phoſoxyd (nitrous gas). The vegetable was pale and ſickly, and water had moſt probably combined with it.

Water then is not decompoſable by the attraction of vegetables for hydrogen, nor is it capable of decompoſition by the attraction of light for oxygen.

The experiments of Prieſtley, Ingenhouz, and Senebier, prove that land vegetables of all kinds produce greater or leſs quantities of phoſoxygen, when expoſed to light in contact with water. I have repeated ſome of the experiments of Ingenhouz, and have obtained nearly the ſame reſults as that philoſopher. I endeavoured to eſtabliſh the fact of the decompoſition of water by light and an organiſed hydrogen attractor by the following experiment.

EXPERIMENT III.

A glaſs cylinder of the capacity of ten cubic inches was filled with mercury. Two ſmall vine leaves were introduced through the mercury, ſo as to detach all atmoſpheric air from them. The mercurial apparatus was now inſerted in a veſſel of cold water. Aqueous gas was paſſed from a veſſel containing water which had been long in ebullition, through a long tube into the cylinder, where it was condenſed by the cold mercury. In this manner the cylinder was filled with water which held no air in ſolution.

The cylinder ſtill inverted in the mercurial trough, was now expoſed to light. In a very ſhort time air globules began to form on the leaves, and in about ſix hours ſufficient was formed to be examined. It meaſured two cubic inches and half, and was nearly pure phoſoxygen.

Since in this experiment no gas of any kind was held in ſolution by the water, and pure phoſoxygen was produced from it, it muſt have ariſen from the decompoſition of the water, by the combination of its oxygen with light, and of its hydrogen with the vegetable.

An immenſe quantity of water then muſt be conſtantly decompoſed by the attractions of land vegetables and of light; and a conſiderable quantity of phoſoxygen muſt be continually ſupplied to the atmoſphere from this ſource.

Carbonic acid, as well as water, is formed in large quantities by combuſtion, fermentation, &c. and is continually liberated during the living action of perceptive beings. Now as this gas is conſiderably heavier than atmoſpheric air, if it was not perpetually decompoſed by ſome means, the lower ſtrata of the atmoſphere would ſoon become compoſed of it, the ſupply of phoſoxygen would be cut off from locomotive organic beings, and perception, thought, and action, would ceaſe to exiſt. As carbon forms one of the moſt predominant principles of land vegetables, we might conclude almoſt without experiment, that the carbonic acid of the atmoſphere is ſomehow decompoſed by them. To determine whether land vegetables were capable of decompoſing carbonic acid by their attraction for carbon, unaſſiſted by the attraction of light for oxygen, I made the following experiment.

EXPERIMENT IV.

A ſmall plant of chironea centaurium growing in a pot of very dry earth was inſerted under mercury, in a jar filled with very pure carbonic acid. It remained in a dark cloſet for four days, and was then examined. The mercury in the jar had aſcended a very little, and the plant had a pale and ſickly appearance. After the carbonic acid was taken up by potaſh, there remained in the jar a very ſmall portion of an incombuſtible gas.

From this experiment it is evident that the attraction of plants for carbon is too weak to enable them to decompoſe carbonic acid; but that they combine with ſmall portions of it, and thus ſuperſaturated with oxygen become white and ſickly.

Light alone effects no decompoſition of carbonic acid, not even in its concentrated ſtate, in the electric ſpark.

To determine whether plants were capable of decompoſing carbonic acid, aſſiſted by the attraction of light for oxygen, I made the following experiment.

EXPERIMENT V.

I planted in each of two pots filled with moiſt earth, a ſmall arenaria tenuifolia. The two plants were of equal weights, and nearly ſimilar. They were inſerted into two jars containing each fourteen cubic inches, one of which was filled with nitrogen, and the other with carbonic acid. The jars were inverted in mercury, and expoſed to the ſolar light for four ſucceſſive days of fine weather in the month of July. The plant in the carbonic acid now looked well and healthy, that in the nitrogen dark and faded. The mercury in the jar filled with carbonic acid had aſcended conſiderably. The gaſes were now examined. In the jar with the carbonic acid there was a deficiency of 2,3 cubic inches. After the carbonic acid was taken up by potaſh, the remaining gas meaſured two cubic inches, and proved to be phoſoxygen. In the jar containing nitrogen, the gas was not diminiſhed more than three tenths of a cubic inch. On the introduction of ſulphure of potaſh a ſmall diminution took place, amounting to ſix tenths of a cubic inch. This diminution was, I ſuppoſe, owing to the abſorbtion of phoſoxygen formed from the decompoſition of the water of the plant.

From this experiment, which I have repeated two or three times on different plants with ſimilar reſults, I conclude that carbonic acid is decompoſable by the force of two attractions; that of light for oxygen, and that of certain organiſed carbon attractors for carbon.

Thus then we find in the vegetable world a ſource for the decompoſition of the carbonic acid and water formed in reſpiration and combuſtion. It is chiefly by the means of vegetables that the equilibrium of the atmoſphere is preſerved, and on their chemical influence, and that of light, do we depend for our exiſtence.

Animals may be conſidered as abſorbing in their reſpiration phoſoxygen, and as taking in in their nutriment, hydrogen, carbon, nitrogen, and oxygen, and giving out perpetually theſe principles in new combinations; water, carbonic acid, and probably ammonia and electric fluid. On this perpetual ſeries of changes their life appears to depend. Vegetables, on the other hand, are continually abſorbing water, carbonic acid, and probably ammonia and nitrogen, and aſſiſted by light in the exerciſe of their organic functions giving out phoſoxygen.

Without vegetables animals would ceaſe to exiſt, and were locomotive perceptive beings removed from the earth, vegetable life would ſoon be at an end.

The vegetation of land plants then may be conſidered as the great ſource of the renovation of the atmoſphere of land animals.

We ſhall proceed to inveſtigate the manner in which the inhabitants of the ocean are ſupplied with phoſoxygen. For as the experiments in my laſt eſſay prove, they continually require it as well as land animals.

I have found by experiments, that water equally diſſolves phoſoxygen and nitrogen, and that nitrogen is not expelled from water by phoſoxygen. The myriads of inhabitants that people the immenſity of the ocean are continually abſorbing phoſoxygen, and giving out carbonic acid, &c.

The ſea water may be conſidered as continually diſſolving atmoſpheric air. Now if there were no ſource for the abſorbtion of the nitrogen and carbonic acid held in ſolution by water, and for the production of a quantity of phoſoxygen equal to that abſorbed by fiſh, the waters of the ocean would ſoon become ſaturated with nitrogen and carbonic acid, and the perceptive inhabitants of it would ceaſe to exiſt.

As land vegetables are the renovators of the atmoſphere of land animals, analogy led me to ſuppoſe that ſea vegetables muſt be the preſervers of the equilibrium of the atmoſphere of the ocean.

I firſt attempted to determine whether like land vegetables, they produced phoſoxygen during the preſence of light; for this purpoſe I made the following experiment.

EXPERIMENT VI.

Into a green glaſs globe containing a hundred and eighty four cubic inches of ſea water, I inſerted a large ſprig of fucus natans, another of conferva fœniculacea, and three or four pieces of an ulva with the name of which I am unacquainted. The globe was expoſed to the ſolar light. It had not been expoſed many minutes, when I beheld with great pleaſure, globules of gas forming on the vegetables, which in a ſhort time were raiſed by them to the top of the globe. In about a quarter of an hour, a large globule was formed, and in about two hours, ſufficient to meaſure. It was one cubic inch and half, and by trial with nitric phoſoxyd, proved to be $\frac{59}{100}$ parts phoſoxygen, and $\frac{41}{100}$ azote.* During this experiment, the therometer ſtood at 47°, and the barometer at 29,9.

Having aſcertained that a compound gas was produced from water by the marine cryptogamiæ, with a larger proportion of phoſoxygen than atmoſpheric air, I made the following experiment to aſcertain whether all the marine cryptogamia produced this gas, and whether it was purer when the diſſolved air was expelled from the water by boiling.

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* A gas indiminiſhable by nitric phoſoxyd.

EXPERIMENT VII.

Into a green glaſs globe containing a hundred and four cubic inches, filled with ſea water previouſly well boiled to expel the atmoſpheric air, I inſerted two cubic inches of fucus fibroſus. The globe was inſerted in a veſſel of boiled water, and expoſed to a bright ſunſhine. Soon after its expoſure the fucus became covered with a vaſt number of globules of air, which raiſed it to the top of the globe, and then detached themſelves to form larger globules. In about four hours ſufficient gas was formed to aſcertain the quantity and quality of it. It meaſured two cubic inches and a quarter. The mercury in the barometer ſtanding at 30 inches, and the thermometer at 54°. By trial with azotic phoſoxyd it proved to be $\frac{71}{100}$ phoſoxygen, and $\frac{29}{100}$ azote.

The globe being expoſed the next day for ſix hours in a very bright ſunſhine, three cubic inches were formed, about $\frac{76}{100}$ phoſoxygen, and $\frac{24}{100}$ azote.

The day after, the globe was expoſed for nine hours and half of moderate weather; three cubic inches and half were formed, of nearly the ſame quality as the laſt.

On the fourth day of this experiment the ſun ſhone very bright for five hours; two cubic inches were formed, $\frac{18}{100}$ azote, and $\frac{82}{100}$ phoſoxygen.

All the gas in this experiment which was not diminiſhed by azotic phoſoxyd, was put into a veſſel, and when examined, proved to be two thirds nitrogen, and one third carbonic acid.

From this experiment it appears that the fuci produce phoſoxygen during the preſence of the ſolar light. The nitrogen produced in this experiment moſt probably aroſe from a ſmall portion of atmoſpheric air, which water ſtill holds in ſolution though boiled.

EXPERIMENT VIII.

Two cubic inches of ulva dichotoma were inſerted into the globe containing a hundred and four cubic inches, filled with water previouſly boiled. It was inſerted in a jar containing water of a ſimilar kind. This globe was expoſed for four days ſucceſſively to the ſolar light. On the firſt day, in eight hours of ſunſhine, three cubic inches and half of gas were formed, $\frac{70}{100}$ phoſoxygen, and $\frac{30}{100}$ azote.

On the ſecond day, when the ſunſhine was uncommonly bright, in ſix hours two cubic inches and half were formed, of $\frac{77}{100}$ phoſoxygen, and $\frac{23}{100}$ azote.

The third day was ſhowry, and the ſun often obſcured; in nine hours but two cubic inches and quarter were formed, containing $\frac{76}{100}$ phoſoxygen.

The ſun was out and very bright continually for ſeven hours on the fourth day; two cubic inches and half were formed, $\frac{83}{100}$ phoſoxygen, and $\frac{17}{100}$ azote.

EXPERIMENT IX.

Into the green glaſs globe of a hundred and four cubic inches, filled with boiled ſea water, two cubic inches of conferva littoralis were inſerted, and the ſame precautions being taken as in the laſt experiment, the globe was expoſed to the ſolar light for four hours; in this time five cubic inches of gas were formed, of the quality of 76.*

The next day the globe was expoſed for ſeven hours of ſunſhine, and ſix cubic inches were formed, of the quality of 78.

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* To prevent unneceſſary repetitions, I ſhall for the future expreſs the quality of gaſes by giving the quantity of phoſoxygen in 100 parts, from whence the quantity of azote will be known.

The day after, the globe was again expoſed for eight hours; five cubic inches were formed, of the quality of 81.

On the fourth day, the weather in the morning was cloudy; but in the afternoon the ſky brightening, it was expoſed for three hours and half, and in this time two cubic inches were formed, about 86.

The gas produced in this experiment, unalterable by nitric phoſoxyd, was chiefly nitrogen.

During the whole courſe of theſe experiments I perceived no ſenſible difference in the production of gas correſponding to difference of temperature.

From theſe experiments it is evident that the different orders of the marine cryptogamia are capable of decompoſing water aſſiſted by the attraction of light for oxygen.

I diſcovered by analyſing the ſea-weeds, that they were compoſed chiefly of hydrogen, carbon, and nitrogen.

As carbon is incapable of ſolution in water, it is evident that the ſea-weeds muſt obtain it from the decompoſition of ſome of its combinations. I found that ſea-weeds placed in water ſaturated with carbonic acid, produced no alteration in it in the dark for ſome days, except that of diſengaging portions of the diſſolved carbonic acid.

To determine their capability of decompoſing it aſſiſted by the attraction of light for oxygen, I made the following experiment.

EXPERIMENT X.

Into a veſſel containing ſixteen cubic inches of carbonic acid, two cubic inches of conferva ſcopularis were inſerted. The veſſel was inverted in mercury, and expoſed to the ſolar light. It remained in a bright ſunſhine for eight hours, and at the end of that time was examined. The gas was diminiſhed to eleven and half cubic inches, ſo that there was a deficiency of five cubic inches and half. Of the eleven and half cubic inches remaining, two were phoſoxygen, the remainder pure carbonic acid.

To determine that the whole of the phoſoxygen produced in this experiment did not ariſe from the decompoſition of the water in the veſſels of the plant, I made the following comparative experiment.

EXPERIMENT XI.

Two veſſels, containing each ten cubic inches, were filled, one with carbonic acid, the other with nitrogen, under mercury. Into each of theſe, ſixty grains of conferva littoralis, previouſly wiped dry, were inſerted. The veſſels were expoſed to the ſolar light for ſix hours, and then examined. The carbonic acid was diminiſhed two cubic inches, the nitrogen not more than one fourth of an inch. One cubic inch and ſeven tenths of phoſoxygen was found mingled with the carbonic acid, with the nitrogen only ſix tenths of phoſoxygen.

EXPERIMENT XII.

Two glaſs globes, containing a hundred and four cubic inches each, were filled, one with boiled ſea water, and the other with ſea water holding in ſolution carbonic acid. Into each of them was inſerted a hundred grains of fucus natans. They were then expoſed to the ſolar light for four hours, and at the end of that time examined. In the veſſel containing carbonic acid and water, three cubic inches of gas were formed, ſixty-four parts phoſoxygen, ſixteen carbonic acid, and twenty nitrogen. In the veſſel with water, one cubic inch and ſix tenths, of the quality of 75.

From theſe experiments it is evident that carbonic acid is decompoſable by the attraction of the marine cryptogamia for carbon, and that of light for oxygen.

Since nitrogen is one of the principles of the marine cryptogamia, and ſince it is held in ſolution by water in a very large proportion in atmoſpheric air, it occurred to me that the marine cryptogamia attract it from the atmoſpheric air diſſolved in the ſea, and combine with it. To aſcertain this I made the following experiment.

EXPERIMENT XIII.

A glaſs jar containing thirteen cubic inches, was filled with nitrogen under mercury. Into this, two cubic inches of conferva purpuraſcens were inſerted, and the quantity of gas in the jar accurately determined. Thus diſpoſed, the whole apparatus was ſuffered to remain in the dark; the mercury in the thermometer ſtanding at 61°. After four days the apparatus was examined; the mercury had aſcended conſiderably, and the gas was meaſured. There was a deficiency of a cubic inch and half. I could not diſcover that any gas had been given out by the plant.

EXPERIMENT XIV.

A ſmall glaſs veſſel containing eight cubic inches, was filled with nitrogen under mercury.

Into this two cubic inches of fucus veſiculoſus were inſerted. The apparatus was expoſed to the ſolar light. After remaining for two days of fine weather, the gas was examined; there was a deficiency of a cubic inch and quarter, and half an inch of the gas remaining was phoſoxygen. A cubic inch and three quarters of nitrogen were conſequently abſorbed by the plant.

The ſimilarity of the conſtitution of the atmoſphere depends on the equilibrium between the gaſes abſorbed, and thoſe generated.

Locomotive perceptive beings are continually abſorbing phoſoxygen in their reſpiration, and giving out carbonic acid, water, &c. Vegetables, aſſiſted by the attraction of light for oxygen, are continually decompoſing water and carbonic acid, of which the oxygen is liberated in combination with light, and the hydrogen and carbon combine with them.

Nitrogen is probably liberated during animal action; and this principle is likewiſe abſorbed by ſome of the vegetables.

The equilibrium of the gaſes held in ſolution by the waters of the ocean is preſerved by the marine cryptogamia claſs of vegetables.

The marine animals are continually abſorbing the phoſoxygen of the atmoſpheric air held in ſolution by water, and giving out carbonic acid, and probably other principles.

The marine vegetables are continually abſorbing the nitrogen of the atmoſpheric air held in ſolution by water; and aſſiſted by the attraction of light for oxygen, decompoſing water and carbonic acid, combining with the hydrogen and carbon, and liberating the oxygen in combination with its attractor light.

Thus the ſea-weeds that every where cover the rocks at the bottom of the ocean, are continually giving out phoſoxygen during the preſence of light. In the deeper parts of the ocean the phoſoxygen, continually formed in ſmall globules, is probably almoſt wholly diſſolved by the ſea-water before it can reach the atmoſphere above; but near the ſea-coaſts where the marine confervæ are found in immenſe quantities, the greater portion of the phoſoxygen formed on their leaves is liberated into the atmoſphere.*

From this diſcovery we are enabled to account for a number of phænomena before inexplicable.

1ſt. In what manner the equilibrium of gaſes diſſolved in the ocean is maintained.

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* The following is an account of the compoſition of the atmoſphere in different parts of Mountsbay in Cornwal, where the experiments on the marine cryptogamia were made. On a calm morning in May 1798, the mercury in the barometer ſtanding at 29,3 and that in the thermometer at 55, the air in the middle of Penzance contained 28,5 per cent. phoſoxygen. The air on the rocks of the ſea-ſhore 30. The air on the ſea about a furlong from the ſhore, nearly the ſame proportion; at a quarter of a mile from the ſhore, the proportion was 30,12. About a mile, 30,1. At two miles 29,12, and at four miles 29. On a calm morning the air at the land's end was 29,13.

2d. Why the ſea air is purer than that of the land.

3d. Why the air near the ſea coaſts is even purer than that on the ocean.

Nature then has catenated together organic beings, and made them mutually dependant on each other for their exiſtence, and all dependant on light.

A privation of light would be immediately deſtructive to organic exiſtence; vegetation would ceaſe; the ſupply of phoſoxygen would be quickly cut off from animals; the lower ſtrata of the atmoſphere would become compoſed of carbonic acid, and perception and volition would exiſt no longer.

The irritability of the living fibre of plants appears to depend on the equilibrium between its principles. This equilibrium is preſerved by light. The principles of the fibre are chiefly carbon, hydrogen, oxygen, and light. Some of them contain portions of nitrogen. The irritable vegetable fibre appears to be a phoſoxyd ſimilar in ſome reſpects to the animal fibre, but containing ſmaller proportions of nitrogen, and light. Different vegetables poſſeſs different degrees of irritability. The mimoſa ſenſitiva the dionea muſcipula, and ſome other plants, poſſeſs an irritability ſo exquiſite as to border on the ſenſibility* of animals. The irritability of the ſea-weed is ſo indiſtinct as to be ſcarcely perceptible. The more perfect land vegetables, poſſeſſed of a vaſcular ſyſtem, require a ſupply of food ſlowly and regularly. They poſſeſs a power of giving out as excrementious, the principles which are noxious to their exiſtence, and by a beautiful œconomy of nature are capable of reproducing their ſpecies.

The marine cryptogamia, in their organization and functions, are very much inferior. Attached to the rocks on the bottom of the ocean, they aſſimilate to them nitrogen and water, and aſſiſted by the attraction of light for oxygen, combine with hydrogen and carbon; they appear to poſſeſs no regular vaſcular ſyſtem for the circulation of fluids, and abſolutely in their generation and growth appear to follow the law of aſſimilation of particles, ſimilar to that by which minerals are cryſtalized.

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* The obſervations of a moſt ingenious philoſopher, Dr. Darwin, render it probable that ſome of the more perfect vegetables poſſeſs ſenſibility. See Darwin's Zoonomia, vol. 1, p. 101, and 102.

The light entering into the compoſition of vegetables appears not only to be the principle on which their irritability more peculiarly depends, but likewiſe to be the principle to which they as well as all ſubſtances chiefly owe the differences of their colors. We do not aſſert that light is the general coloring principle; this would be abſurd in the preſent ſtate of our knowledge. We ſhall attempt to demonſtrate that many vivid colors depend on combined light.

Almoſt all the ſimple ſubſtances and the combinations of the ſimple ſubſtances that contain no light, are either pellucid, white, or black. Among theſe are the gaſes, water, alcohol, the acids, the alkalies, the pure earths, phoſphorus, ſulphur, when in its pure ſtate and uncombined with light,* carbon, and the different metallic ſubſtances which in general are either white, or black and white, that is, grey. Copper, which is yellow, appears to contain light, and probably ſome other metals.

The compounds into which oxygen enters, and which contain no light, as oxyds, acids, &c. are likewiſe generally either tranſparent, white, or black; tranſparent, as the ſimple acids and ſome of the oxyds, white, as the oxyds of lead, zinc, and antimony, black, as the oxyd of iron.

The inorganic compounds containing phoſoxygen, are almoſt all of vivid colors; and theſe colors appear to be in ſome meaſure correſpondent to the quantities of light entering into their compoſition. A ſmall quantity of light combined with oxygen and metallic bodies, renders them either brown, as the argentic and ferric phoſoxyds,* or purple, as the auric phoſoxyd. Bodies containing a larger proportion are either green, as the cupric phoſoxyd, or yellow, as the tungſtic phoſoxyd. The phoſoxyds containing a ſtill larger portion of light, are bright red, as the plumbic and mercuric phoſoxyds. The chromic phoſacid, as was obſerved in the laſt eſſay, likewiſe owes its color to combined light.

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* Sulphur, in its common ſtate, evidently contains a portion of light, as is evident from the experiment mentioned in that part of the laſt eſſay which relates to phoſphoreſcent bodies.

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* In my laſt eſſay, treating of the phoſoxyds, I have neither mentioned the ferric, cupric, or blue cobaltic phoſoxyds, becauſe I am poſſeſſed of no facts which abſolutely prove the preſence of light in theſe bodies. This however is extremely probable, not only from analogy, but from the phænomena obſerved in their oxydation. The variety of colors of the oxyds of iron combined with earths &c. depend moſt probably on the different quantities of light entering into their compoſition.

The phoſoxyds loſe their colors when light is ſubtracted from them in combination with oxygen.

When the tungſtic phoſoxyd is expoſed to light, it gives out phoſoxygen, and turns from yellow to blue.

The mercuric phoſoxyd, on the ſubtraction of a portion of its light in combination with oxygen, becomes brown.

The plumbic phoſoxyd likewiſe becomes brown on a ſubtraction of a portion of its light and oxygen; when the whole of its light is ſubtracted, it becomes white.

The ſulphuric acid combines with the plumbic oxyd, but not with the plumbic phoſoxyd. When it is poured on the laſt, phoſoxygen is given out, the phoſoxyd gradually loſes its color with its light and oxygen, becomes white, is converted into an oxyd, and combines with the acid.

The green pruſſiate of iron, on a ſubtraction of oxygen and light from it, becomes blue.

The whiteneſs of etiolated vegetables is occaſioned by the deficiency of light; the different ſhades of green in the leaves of vegetables depend on the light entering into their compoſition; and the fine colors of the different flowers appear to be produced by combined light, as will appear from the following experiments and obſervations.

EXPERIMENT XV.

Two lettuces of equal ſize, both fine, healthy, green plants, were planted in two pots filled with moiſt earth; one of theſe plants was inſerted in a jar filled with carbonic acid, and deprived of light. The other was expoſed to light and atmoſpheric air. In about twelve hours the inferior leaves of the plant in the dark began to fade, which obliged me to remove it out of the jar filled with carbonic acid, and expoſe it to atmoſpheric air. It remained deprived of light for ſix days, during which time it was plentifully ſupplied with water. At the end of this time the leaves were very pale, the lower ones perfectly white; on the upper ones a few ſpots of green remained. It was again deprived of light, and daily ſupplied with water. After remaining for a week it was again examined: the leaves were now quite white.

The plant which had been expoſed to light and air was now examined. It was of a fine lively green, and much larger than the etiolated plant. Equal weights of the leaves were analiſed by deſtructive diſtillation. The only perceptible difference in the products was a larger proportion of carbonic acid and water in the white, and in the green more hydrogen and reſidual carbon. I obtained from ſome of the remaining green leaves, by a low heat, a ſmall quantity of phoſoxygen mingled with carbonic acid; from the white, carbonic acid alone.

I have often obtained from the green leaves of vegetables, by applying a low heat very gradually, ſmall portions of phoſoxygen; from which it appears that the green colors of vegetables depend on the light, or the phoſoxygen entering into their compoſition, and the whiteneſs of etiolated plants, to the deficiency of light in their compoſition.

Plants, in the proceſs of etiolation, loſe the light combined with their leaves, and become white. They are as well capable of combining with a larger proportion of light, and thus ſuperſaturated with light, become bright colored red or orange, as will appear from the following experiment.

EXPERIMENT XVI.

I procured two lettuces and two plants of ſorrel (rumex acetoſa) of nearly the ſame ſize. One of each kind was planted in moiſt clay, which had been before proved to be fit for vegetation, the other in ſilicious ſand mingled with a little clay. The plants in the clay were placed under a ſhady wall and daily watered, thoſe in the ſand were placed in a ſituation conſtantly expoſed to the ſun, and were ſupplied with very ſmall quantities of water. In about ſix days the plants expoſed to light became ſpotted with red and orange in many parts of their leaves. Theſe ſpots continued to increaſe in number and ſize, and in about a fortnight the color of the upper parts of the leaves of the lettuce were changed to a dark red. The upper parts of the leaves of the rumex acetoſa were red, and the lower of a duſky green. The plants in the ſhade were of a fine green. Comparative analyſes of equal parts of the green and the red leaves were made. The reſults of three experiments were, that the red leaves contained more phoſoxygen, hydrogen, and reſidual carbon, and the green more carbonic acid and water.

From this experiment it is evident that the red or brown colors of leaves too much expoſed to light, is owing to their containing a ſuperabundance of light, which in the analyſis combined with oxygen.

The different colors of the leaves of different plants then, moſt probably depend on the different quantities of light, oxygen, and carbon, entering into their compoſition.

Flowers, like leaves, depend for their vivid colors on the light entering into their compoſition. The red flowers, as the roſe, anemone, &c. appear to contain the greateſt quantities of light. I have found by experiment that red roſe trees, when carefully included from light before their flowers begin to appear, and ſupplied plentifully with water and carbonic acid, produce flowers almoſt white.

By making comparative analyſis of the white and red roſes, from ſeveral experiments, I think I have every reaſon to conclude that the red roſes owe their colors to combined light.

By diſtilling muriatic acid from red roſe leaves, I diſcovered that a ſmall portion of the acid was converted into muriatic phoſacid, which proves that the red roſe leaves contain light and oxygen.

The pink, orange colored, and yellow flowers appear to contain ſmaller proportions of light; the dark purple and blue ſtill leſs. In the white flowers I have never been able to detect the preſence of light. The fine colors of fruits, which depend entirely on their expoſure to light, moſt probably depend, like thoſe of flowers, on the light entering into their compoſition.

I have made ſome experiments on the marine cryptogamia, which ſeem to prove that they are governed by the ſame laws of color as land vegetables. I have ſucceeded in blanching the dark colored confervæ by ſecluding them from light, and their different colors appear to depend on the different quantities of light, oxygen, and carbon, entering into their compoſition. Zoophyta and fiſh, the perceptive inhabitants of the ocean, appear to depend on ſimilar cauſes for their colors. I have obſerved that the zoophyta expoſed to light are uniformly brighter colored than thoſe which have been by any means ſecluded from it, and I ſucceeded in altering the colors of two ſea anemones from a dark red to a pale pink, by ſecluding them from light.

The parts of fiſh which are expoſed to light, as the back fins, &c. are uniformly colored red, brown, green, yellow, blue, &c. in different fiſhes; but the belly, which is deprived of light, is uniformly found white in all of them.

Rational analogy alone would induce us to ſuppoſe that the colors of land animals depend upon the ſame cauſes. Independant of this, we have facts and experiments ſufficient to eſtabliſh this truth.

The birds that inhabit the tropical countries are much brighter colored than thoſe of the north. Thoſe parts of birds which are not expoſed to light are uniformly pale. The feathers on the bellies of birds are generally pale or white: the back, which is expoſed to light, is almoſt always colored: the breaſt, which is partially expoſed in moſt birds, is brighter than the belly, and paler than the back. Thoſe parts of the ſame feather which are expoſed to light, are uniformly bright colored, when compared with thoſe parts that are deprived of its influence.

The colors of quadrupids depend on ſimilar circumſtances. The beaſts of the equatorial countries are uniformly brighter colored than thoſe of the polar countries. The hair on the parts of beaſts not expoſed to light is uniformly paler than that covering the parts expoſed to its influence; and it affords a ſtriking proof of the truth of this theory, that ſome of the northern animals are dark colored in ſummer, and white or pale in winter.

The human being is equally dependant for its color on the influence of light.

The color of the ſkin depends on the color of the rete mucoſum. This is white in the people that inhabit the north of Europe, copper colored in the American, and black in the Negro. The cuticle, which is of the ſame color, and equally pellucid in the different ſpecies of mankind, appears to contain no oxygen or light, and is moſt probably compoſed of carbon, hydrogen, and nitrogen. The rete mucoſum is probably compoſed of carbon, oxygen, hydrogen, and nitrogen. The comparative quantities of carbon and oxygen in it appear to occaſion the differences of its colors. Theſe quantities depend on the quantity of oxygen attracted from it by light.

Light acting on the rete mucoſum of the African, is continually ſubtracting oxygen, the principle to which its whiteneſs is owing. When the oxygen is ſubtracted, the carbon becomes the predominant principle, and hence that blackneſs peculiar to the negroes and the inhabitants of the torrid zone. In the Americans, the inhabitants of Aſia, and the ſouthern Europeans, the color varies from dark copper colored to pale tawny. Theſe people are leſs expoſed to light than the Negroes, conſequently their ſkin contains a larger proportion of oxygen. The inhabitants of the northern countries, who are ſtill leſs expoſed to light, are white: their rete mucoſum contains its full proportion of oxygen.

In Europeans we find the parts of the body expoſed to light, darker colored than thoſe that are covered.

Women, who are leſs expoſed to light, are fairer than men.

A ſubtraction of oxygen from the rete mucoſum by any means, uniformly blackens it. The application of ſulphure of potaſh blackens the ſkin almoſt inſtantly, as I have found by experiment. By combining with oxygen the rete mucoſum is uniformly whitened. Dr. Beddoes whitened the fingers of a Negro by muriatic phoſacid, which appears capable of giving out a ſmall portion of oxygen and of ſtill retaining all the light entering into its compoſition.

The different colors of different parts of the organic matter of the body depend chiefly on the light and oxygen entering into their compoſition.

The red muſcles, as we attempted to prove in the laſt eſſay, are phoſoxydated compounds, and probably owe their color to the light entering into their compoſition. From the white color of the nerves, and their office of conducting, and being excited into ſenſible action by condenſed light, we may conclude that they contain no light in their compoſition. The arterial blood owes its fine color to the light and oxygen entering into its compoſition; the venous blood is black from a deficiency of light and oxygen and a ſuperabundance of carbon.

The colors of the cheeks depend on the number of blood veſſels in the cutis, the quantity of blood circulating through them, the color and thickneſs of the rete mucoſum and the thickneſs of the epidermis. Hence thoſe people who have a thin epidermis, a white rete mucoſum, and a number of blood veſſels in the cutis, have very roſy cheeks. When the veins on the ſurface of the cutis are more numerous than the arteries, the color of the cheeks will approach more to dark red or purple; when, on the contrary, the arteries are more in number than the veins, the color will approach nearer to fine vermilion, which is the hue of health.

Whatever increaſes the general action of the arterial ſyſtem, reddens the color of the cheeks: general ſtimuli produce this effect. Certain paſſions of the mind more peculiarly occaſion an increaſed action of the veſſels of the ſkin of the cheeks.

Though the differences of the color of the ſkin in different nations muſt have originally depended entirely on the chemical influence of light; yet when theſe colors are once produced, their changes are in ſome meaſure dependant on the mind. When certain colors are conſidered as beautiful, the generating imagination makes them hereditary, and the chemical changes from the influence of light are more ſlowly produced. Thus Europeans, though expoſed to light in the African countries, do not become black, but in a great length of time; and Negroes, though deprived of light, their accuſtomed oxygen attractor, are not blanched for many generations.

ADDENDA.

The experiments on the generation of heat were made long before the publication of Count Rumford's ingenious paper on the heat produced by friction. His experiments alone go far to prove the non-exiſtence of caloric, and when compared with the ſecond and third experiments in my eſſay, will, I ſhould conceive, leave no doubts on the mind of the impartial and philoſophic reaſoner.

As vegetables depend for their life on a certain equilibrium between their principles, they muſt be continually ſupplied with the different ſubſtances by which this equilibrium is maintained.

A ſupply of water, carbonic acid, light, &c. is eſſential to the true vegetative proceſs. The greater number of vegetables die in a ſhort time, when deprived either of water or carbonic acid.

I ſhould ſuppoſe, that if it were poſſible to deprive carbonic acid, hydrogen, and nitrogen, of the water they generally hold in ſolution, they would be ſoon deſtructive to all vegetables. The gaſes that I made uſe of in my experiments probably contained much water, as I always filled my veſſels under water, and then tranſferred them to a mercurial trough, the ſurface of which was covered with water ſaturated with the gas with which the experiment was to be made. After the inſertion of the plant, the height of the mercury was marked; from whenc the abſorbtion of gas could eaſily be known.

In carbonic acid confined in this manner under mercury, many vegetables live for a conſiderable time. Among theſe vegetables are the different ſpecies of arenaria, the chironea centaurium, and the ſedum anglicum.

As I was obliged to be methodical in my laſt eſſay, I have only mentioned one experiment to prove the decompoſition of carbonic acid by plants, that on the arenaria tenuifolia, becauſe it was the moſt concluſive. I do not, however, ſuppoſe that the whole of the phoſoxygen produced in that experiment aroſe from the decompoſition of carbonic acid; ſome of it probably aroſe from the decompoſition of the water diſſolved by it.

Some plants die in carbonic acid in ten or twelve hours; among theſe are water plants in general, and the different ſpecies of rumex.

The generation and growth of vegetables, though apparently very complex proceſſes, depend on the ſimple laws of attraction and repulſion.

The apparent regular expanſion of a vegetable germ is produced by its power of attracting and aſſimilating to itſelf a number of principles. Vegetation only differs from cryſtalization, as being a more complex proceſs. In cryſtalization certain particles, homogeneal in relation to each other, combine together according to the laws of their reciprocal attractions and repulſive motions, to form regular figures. In vegetation, a compound ſubſtance (that is, a germ) is capable of attracting different principles. When theſe principles are aſſimilated to it, it poſſeſſes the power of attracting new ones, and thus ſhoots out into leaves of different forms, and continually gaining new attractive powers, at length produces flowers and fruits.

To illuſtrate this, we will ſuppoſe a germ of a certain figure, compoſed of carbon, hydrogen, and oxygen, in certain proportions. The combined attraction of hydrogen and carbon for oxygen, and that of oxygen for hydrogen and carbon, enables the germ to combine with water and carbonic acid. Theſe principles become aſſimilated with it, it conſequently encreaſes in bulk, and riſes above the ſoil which gave it birth. When acted on by light, a new chemical proceſs takes place; the ſuperabundant water and carbonic acid, the oxygen of which rendered it white, are decompoſed by the attraction of light for oxygen, and probably by the mutual attractions of hydrogen and carbon for each other. A new equilibrium of principles now takes place; the plant becomes capable of combining with portions of phoſoxygen, and its color gradually changes from white to green. In this manner the vegetative proceſs proceeds, from the aſſimilation of new principles new attractive powers are continually gained, and different parts of the plant become of different forms and colors.

It is probable that the proceſs of animalization is ſimilar to that of vegetation, and different only as being more complex. Indeed, as the principles of living beings become more numerous, the delicacy of organization increaſes.

The combination of particles, homogeneal in relation to each other, produces regularity of form. The combination of different principles, according to certain laws of attraction, produces irritability. Senſibility may be the reſult of an infinitely more complex organization.

Almoſt all the vivid colors with which we are acquainted, belong to the ſubſtances which contain light. We do not, however, mean to aſſert that color is a neceſſary effect of combined light; on the contrary, many bodies that contain a conſiderable quantity of light in their compoſition, are white, black, or pellucid; white, as the phoſnitrates, black, as the phoſoxyd of manganeſe, and pellucid, as the phoſacids.

To call any principle the coloring principle, is abſurd. The colors of bodies, as we attempted to prove in the doctrine of light, depend on their capacities for ſubtracting the repulſive motion of light. The colors of compound ſubſtances are found in general to be totally different from, and to bear no relation to the colors of their conſtituents. Two tranſparent ſubſtances combining, often produce an opaque body; and two white conſtituents, a vivid colored compound.

ESSAYS

ON

HEAT, LIGHT, and the Combinations of LIGHT,

WITH A NEW THEORY OF

RESPIRATION.

ON

The Generation of OXYGEN GAS,

AND THE CAUSES

OF THE COLORS OF ORGANIC BEINGS.

By HUMPHRY DAVY.

> " La lumiere, ses combinaisons sont encore moins connues
> " que celles du calorique.
>
> " L'organisation, le sentiment, le movement spontané, la
> " vie n'existent qu'a la surface de la terre, et dans les lieux
> " exposés a la lumiére."
>
> *Lavoisier. Traité elementaire.*

SPECIMEN

OF

An ARRANGEMENT *of* BODIES

ACCORDING

To their PRINCIPLES.

By the EDITOR.

Specimen

Of an Arrangement of BODIES *according to their* PRINCIPLES.

CLASS I. LIGHT.
CLASS II. OXYGEN.
CLASS III. PHILOXYGENA.
CLASS IV. MISOXYGENA.

N. B. Philoxygena are s s. combinable with oxygen or with phoſoxygen or with both; i. e. combuſtible; oxydable; acidifiable.
Misoxygena are s s. uncombinable with oxygen or with phoſoxgen; i. e. non-oxydable; non-acidifiable; incombuſtible.

I. LIGHT.
Electric fluid.
Galvanic fluid.

II. OXYGEN.
ox. with light;
i. e. phoſoxygen.

III. 1. HYDROGEN.
Hyd. with oxygen;
i. e. water.

2. AZOTE.
Az. with phoſ-oxygen
i. e. atmoſph. air,
or gazeous oxyd,
or nitrous air,
or nitrous acid,
or nitric acid.

Az. with hydrogen;
i. e. ammonia.

3. CARBON.
(qu. Azote with
hydrogen?)

C. with oxyg.
i. e. carbonic acid.

C. with hydrogen;
i. e. carbonated hydrogen,
or alcohol,
or æther,
or oils.

4. SULPHUR.
(qu. compoſition?

S. with oxygen;
i. e. ſulfureous or
ſulfuric acids.

S. with oxygen
and phoſoxygen,
i. e. phoſoxygenated
ſulfuric acid?

5. PHOSPHORUS.
(qu. compoſition?)

Ph. with oxygen;
i. e. phoſphoreous,
or phoſphoric acids.

Ph. with hydrogen;
i. e. phoſphorated
hydrogen.

Ph. with oils,
&c. &c.

6. MURIATIC RADICAL
(qu. hydro-azote?)
with oxygen
i. e. muriatic acid.
with phoſoxygen
i. e. phoſoxygenated
muriatic acid.

7. HYDRO-CARBON
(hydro-carb-azote?)
with oxygen;
i. e. acetous
pinguedinous,
tartarous,
oxalic,
&c. &c. acids.
qu. does not hydro-carbon,
as a compound baſe, always expel light?

7. Platina.
8. Gold.
combinable
only with
phoſoxygen?

9. Silver.
10. Quickſilver.
11. Lead.
12. Manganeſe.
combinable
with oxygen
and phoſoxygen.

13. Copper.
14. Iron.
&c. &c.
&c. &c.
qu. as to
the oxydation
or phoſoxydation
(or both) of
other metals.

Metallic s s. that is, compounds of hydrogene and azote.

IV. 1. Barytes.
2. Strontites.
3. Potaſh.
4. Soda.
5. Lime.
6. Magneſia.
7. Alumine.
8. Jargonites.
9. Silex.
&c. &c.

Alkalis.

Does the mode of union of their elements render them non-oxydable? or have they already oxygen or phoſoxygen cloſely combined?

REMARKS

ON THE

PRECEDING TABLE.

ITS ORIGIN.

This ſcheme was firſt printed in ſpring 1798, in illuſtration of part of a courſe of chemical lectures, which the deſire of philoſophical information, manifeſted by many perſons at Briſtol, induced the editor to undertake.

It was my deſign to preſent a number of examples, juſt ſufficient to render my ideas intelligible; by no means to digeſt all chemical facts into a table. The only alterations made for the preſent impreſſion, are thoſe which the diſcoveries related in the foregoing papers rendered neceſſary.

WHY CALORIC IS OMITTED.

Long before I was acquainted with theſe diſcoveries, or Count Rumford's experiments on friction, I had expunged the *matter of heat* or *caloric* from my chemical ſyſtem. And on this occaſion it is but juſtice to atteſt, that the author of the former derived no aſſiſtance whatever from the Count's ingenious labours. My firſt knowledge of him aroſe from a letter written in April 1798, containing an account of his reſearches on heat and light: and his firſt knowledge of Count Rumford's paper was conveyed by my anſwer. The two eſſays contain proofs enough of an original mind to make it credible that the ſimple and deciſive experiments on heat were independently conceived. Nor is it neceſſary in excuſe or in praiſe of his ſyſtem to add, that at the time it was formed the author was under twenty years of age, pupil to a ſurgeon-apothecary in the moſt remote town of Cornwal, with little acceſs to philoſophical books, and none at all to philoſophical men.

What induced me to reject the almoſt univerſal opinion concerning caloric, was principally the ſtrange abuſe of the doctrine of *latent heat* by Mr. Lavoiſier. This I conſider as a compleat *reductio ad abſurdum* of the hypotheſis of caloric, and a humiliating example of the frequent inability obſervable in men of the moſt energetic underſtanding, to puſh their ſcepticiſm far enough. Experiments on the capacity of bodies for heat, demonſtrate that there cannot be condenſed in nitre enough of caloric for that generation of gaſes and increaſe of temperature which are obſervable in the firing of gunpowder and of other exploſive compoſitions. If there occur propoſitions in the ſame ſyſtem not quite ſo glaringly contradictory, ſtill it cannot be maintained without many aſſumptions perfectly gratuitous. And, unleſs we ſuppoſe a change of capacity, or the condenſation of oxygen, the phænomena attendant upon friction and percuſſion appear totally inexplicable. To poſterity thoſe pages in the hiſtory of ſcience will not appear the leaſt extraordinary, which exhibit almoſt all the philoſophers of our innovating age as more than acquieſcing in an old theory or arrangement of facts which excludes the moſt capital of all.

Moſt of the phænomena relative to heat may be claſſed under five heads.

1. The change of bulk, for the moſt part correſpondent to the variations of a particular ſenſation; and the difference obſervable in different bodies in this reſpect.

2. The manner in which adjacent bodies, unequally heated, affect each other's temperature, or the conducting power for heat.

3. The effect of the mixture of bodies unequally heated; or the doctrine of capacity.

4. The change of ſtate in bodies in conſequence of variation of temperature.

5. The excitation of heat; and particularly the relation of heat and light.

All theſe phænomena I found much more eaſily reconcileable to the *mechanical* than the *chemical* doctrine of heat.

LIGHT.

The part of my table belonging to light, is new. Before I was acquainted with the two preceding papers, I had no ſatisfactory ideas on the chemical affinities of this great agent. What I have now received into the arrangement depends upon the evidence contained in thoſe papers. I had before conſidered light and the electric fluid as identical; and it is probable that the galvanic influence depends upon light attached to animal ſubſtances, as the electric fluid is found attached to ſome inanimate ſubſtances; and that it is excitable in a manner ſomewhat different. That the galvanic and electric fluids differ in their laws of motion, has been rendered probable by the reſearches of Dr. Fowler, of Dr. J. F. Ackerman, (Verſuch uber die Lebenſkrafte organiſirter Korper Frankfurt. 1797), Mr. Humboldt (Verſuche uber die gereizte muſkel und Nerven-faſer. Poſen. 1797), and others.

OXYGEN.

Oxygen, as well as the modifications of light muſt, I believe, be claſſed apart. So little analogy does their chemical action bear to that of any other ſubſtance.

CLASS III. AND IV.

I could think of no principle by help of which ſo conveniently to arrange other ſubſtances as their relation to oxygen and phoſoxygen. I doubt whether any arrangement will ever be deviſed, equally ſuited to the moſt numerous and eſſential chemical properties of bodies. Whatever arrangement be obſerved, it will happen here as in the botanical indexes of Linnæus and other naturaliſts; like bodies will ſometimes be ſeparated, and the unlike be brought near. The greateſt blemiſh in the preſent table ſeems to me the disjunction of ammonia from potaſh and ſoda; but we have the advantage of claſſing potaſh and ſoda with the other fixed alkalies (namely barytes, ſtrontites, and lime), from which they have heretofore been very unphiloſophically parted. Dr. Cullen's diſtribution of the objects of chemiſtry into ſalts, earths, inflammables, metals, and waters, has obtained moſtly in this country in conſequence of its adoption by Dr. Black. It was well ſuited to the ſtate of knowledge for the time being. The claſſification of Fourcroy and the modern foreign writers of compends of chemiſtry exhibits, in my opinion, ſmall ingenuity or comprehenſion. The phænomena in which light and oxygen are concerned, eſpecially as the actions of life promiſe to be comprehenſible under them, will probably long conſtitute the moſt curious and important part of chemiſtry. And if they afford a convenient and extenſive principle of arrangement, it ought unqueſtionably to be followed. Thoſe who compare it more ſtudiouſly than I have done with the whole maſs of facts, will apply it more happily in the ſubdiviſions.

More than mere claſſification, I had it in view to place under the reader's eye certain probabilities that might lead to the analyſis of different bodies, at preſent conſidered as ſimple.

CARBON.

Mr. W. Henry's late experiments ſeem to have invalidated the concluſions of Dr. Auſtin. Neverthleleſs, I have placed a query after *carbon.* No poſitive effect adequate to the immenſe proviſion of azote can, I believe, be pointed out, if it be not abſorbed by vegetables to form charcoal. It may be queſtioned whether the decompoſition of carbonic acid be equal to the ſupply of that conſtituent part. This however is a mere ſurmiſe.

Experiments on vegetation in which the atmoſphere ſhould be excluded, and the water ſhould be confined by quickſilver, would throw light on this queſtion, becauſe then the acceſs of charcoal and carbonic acid might be prevented, and only known ſubſtances admitted. There is another reaſon for ſuſpecting charcoal to be a compound. If phytivorous animals do not take in azote by reſpiration, they may obtain it from the charcoal of the plants in which they feed. From the plants they muſt obtain it.

If we could fully rely upon the experiments, from which it has been inferred that organized beings, both animal and vegetable, can ſubſiſt upon air and water, (and if atmoſpheric air, conſiſting of phoſoxygen and azote, is to be underſtood), it would be probable that ſulphur and phoſphorus conſiſt of hydrogen and azote. For animal ſubſtances contain phoſphorus, and they yield ſulphur during putrefaction. I am not ſure, however, that this latter fact has been aſcertained with reſpect to the ſubſtance of animals confined to air and water. In ſuch a caſe, however, I ſuppoſe we may argue from analogy.

SULPHUR AND PHOSPHORUS.

The fuſibility of ſulphur and of phoſphorus appears to me to afford ſome hope of their decompoſition. It certainly renders them much more manageable by the chemiſt than charcoal. A project for this purpoſe ſome time ago occurred to me; and if no one publiſhes an account of the failure or ſucceſs of experiments ſuch as I have in view, ſomething on this head may appear in the next volume of the preſent collection.

MURIATIC RADICAL.

The ſummary which Dr. Girtanner has publiſhed of his experiments (Antiphlogiſtiſche Chemie; Berlin. 1795. s. 154), goes a very little way towards proving that hydrogen is the baſe of the muriatic acid. But this acid is copiouſly formed in nitre-beds; and (what is more in point), the product of experiments in which Dr. Prieſtley fired hydrogen and oxygen with ſome azote in cloſe veſſels, was found by Mr.

Keir to be muriatic with nitrous acid and water. Hence I ſuſpect the muriatic to be a compound radical. Certainly if we can rely upon the purity of the ſubſtances employed by Dr. Prieſtley, it muſt be hydrogen or azote, or compounded of both. That it is not hydrogen alone may in ſome meaſure be preſumed from the reſult of thoſe laborious experiments in which hydrogen and oxygen gas, as free from azote as they could be procured, were burnt together without any apparent production of muriatic acid.

FLUORIC AND BORACIC RADICALS.

Having nothing particular to obſerve of theſe, I have not placed them in the table.

METALS.

In conformity with the above related experiments on light and oxygen, and alſo with the principle of this arrangement, I have thrown the metals into three ſubdiviſions. The firſt group contains ſuch as ſeem to combine with phoſoxygen only: viz. platina and gold. Silver, lead, and mangane ſe, evidently combine with oxygen and phoſoxygen; the firſt being ſoluble in ſulfuric acid, and the two others yielding phoſoxygen (oxygen gas) on application of heat, without reduction. The nature of the reſt is uncertain. Every fundamental diſcovery neceſſitates a reviſion of facts; and it is to be hoped that Mr. Davy himſelf will find leiſure for a train of experiments on oxydation and phoſoxydation.

Concerning the compoſition of metallic ſubſtances, not much can be ſaid. Whether to create a diverſified ſyſtem of bodies out of one, or out of a few or many elements, imply moſt wiſdom and power, is a queſtion which different perſons would decide according to their various taſte in world-making. And the diſpute might go on without hope of termination for as many centuries as have elapſed ſince Thales the Mileſian to the preſent hour. But we have ſome indications that the metals are not ſo many ſimple ſubſtances; and in the caſe of ſome among them, by accurate *cloſe* experiments on organiſed bodies, we might have certain proofs.

The exiſtence of iron in ſuch variety of plants and animals; and of manganeſe in ſome plants, ſuggeſts an opinion that theſe metals are compounded by the organic powers; and then we are warranted by analogy in ſurmiſing that the other metals conſiſt but of the ſame principles differently modified.

CLASS IV.

If future experimenters ſhould accompliſh the oxydation of any of the bodies of the fourth claſs, ſuch bodies muſt be transferred to the third claſs. Should it be diſcovered that oxygen enters into their compoſition, the terms *philoxygenous* and *miſoxygenous* muſt be changed; and it is probable that in the preſent ſtate of facts, more apt titles might be found for the two laſt claſſes.

POTASH AND SODA.

The exiſtence of ſeveral of the bodies themſelves of the third and fourth claſſes, or of their elements, in ſo many organiſed beings, has often ſuggeſted the idea of the formation of the bodies by the chemical proceſſes of life. Thus a late author: " Theſe experiments, I " think, ſhew that vegetables and animals " poſſeſs organs, capable of aſſimilating to their " own nature, the matter deſtined to their " nutrition ; that the animal powers can pre- " pare ſalt, lime, and iron, which are found to " exiſt formally in the body ; however the " greater part of an animal or vegetable is with- " out ſuch ſubſtances, yet, when deſtroyed by " fire, its component parts do in general re- " combine, and thus produce thoſe ſubſtances. " Since, then, animal matter is only a peculiar " arrangement of common matter, why may " not the organs of imperfect animals be capa- " ble, as well as vegetables, of producing this " arrangement? It ſeems probable that they

“ are capable; but animals of more perfect “ organization who poſſeſs ſenſation, and were “ deſigned by nature to live on previouſly pre- “ pared animal and vegetable matter; they “ appear leſs able to accompliſh ſuch converſion, “ and die ere it is effected, from the derange- “ ment of their ſenſitive organs.” (Abernethy's Eſſays. 1793. p. 102, 103.) Again: “ I had “ imbibed the opinions of the great philoſophers “ of this iſland, who from reflection and rea- “ ſoning were induced to believe that the ulti- “ mate particles of matter were the ſame; and “ that the various ſubſtances with which this “ world preſents us, were only differences in “ the arrangement and motion of ſimilar par- “ ticles. The teſtimony of experiment appears “ to me to be now added to the truth of an “ opinion, formerly ſupported merely by the “ ſuggeſtions of reaſon.” (Ibid p. 104.)

The opinion has ſufficient plauſibility to deſerve to be properly aſcertained; but it has ſtill I fear no ſupport but in conjecture, which is probably all to which “ ſuggeſtions of reaſon" amount. If however the gold fiſh which Dr. Fordyce kept for ſix months in diſtilled water were placed in the ſame ſituation as the ſubjects of Mr. Abernethy's obſervations, it can by no means be concluded that they ſubſiſted on atmoſpheric air and water. The atmoſphere, being a vaſt magazine of duſt, vapours, and gaſes, may yield to plants almoſt whatever they can draw from the soil.

Whether ſome of the matters of the fourth claſs exiſt in organized bodies, or are formed by proceſſes in which no material is added, is doubtful. Nor is it of any conſequence to the hypotheſis. Thoſe matters will in the latter caſe equally conſiſt of the elements that enter into organic bodies.

Some recent naturaliſts have adopted the doctrine of equivocal generation in conſequence of their own obſervations. Among others, Leſke, in his travels through Saxony *(Reiſe durch Sachſen)*. If there ariſe, in the viciſſitude of things, equivocal maſſes capable of arranging the firſt principles of bodies into organic compounds, theſe, if not neceſſary to the exiſtence of vegetables, may be expedient to their flouriſhing ſtate. The effect of ſome manures ſeems to depend on the preparation of nutriment. There are facts which shew that the organic powers, rather than form certain conſtituent parts, will receive them when preſented ready formed. Sometimes they receive them in a compound ſtate, and afterwards decompoſe them, rather than compound them anew, though capable of doing ſo. This is true of that product which in whatever ſtate it exiſts in vegetables, appears as alkali after incineration. Thus Jacquin, in his elements of chemiſtry, informs us that the ſalſola kali, which in maritime ſituations and in ſalt ſoils yields ſoda (probably from the decompoſition of muriate of ſoda), when growing ſo far inland as the vicinity of Vienna, yields potaſh.

LIME AND SILEX.

On the probability of the organic formation of lime, alumine, and ſilex, it is needleſs to expatiate. The facts which favour ſuch an opinion cannot be unknown to any ſtudent of chemiſtry.

In a paper in the *Journal de phyſique*, Mr. Dolomieu has endeavoured to ſhew by experiment that hydrogen enters into the compoſition of ſilex. Several years ago, however, I kept ſmall pieces of ſilex boiling for a conſiderable time in heated oxygen gas. No combuſtion was obſerved, and the ſeveral pieces weighed as much as before they were heated. They had only acquired a ſpungy texture. Evidently as they appeared to bubble while hot, and to be porous when cooled, not a particle ſeemed to evaporate at this high temperature. The opinion of the compoſition of flint therefore, at preſent ſeems to have no foundation but in the well-known facts relative to the bamboo.

Poſſible uſe of the preſent Conſpectus.

Should the preſent view lead, in but a ſingle inſtance, to ſucceſsful experiments on the decompoſition of bodies of unaſcertained conſtitution, it would be a great advance towards the removal of the preſent difficulties in chemical theory and practice. Theſe difficulties are chiefly of two kinds. On the one hand we are limited in our power to change the forms of ſubſtances by the firm, and hitherto impenetrable ſtructure of metals, earths, and of ſome other bodies. Their decompoſition would doubtleſs be followed by the creation of numberleſs new arts, and a great change in the condition of man.

The other ſpecies of difficulty is much more worthy of the efforts of the enterpriſing and the ingenious. To overcome it would contribute infinitely more to the advantage of human ſociety. This difficulty is formed by the complicated and ever-changing texture of living matter. Sound philoſophy leads us to ſuppoſe that here, as in inanimate matter, action depends upon compoſition; and variety of action, of courſe, upon variety of compoſition. In the conſtitution and qualities of nerves and muſcles, there may poſſibly, within the limits of the living ſtate, exiſt as wide a difference (I do not ſay the ſame difference) as between the *nitric* and *nitrous* acids. When we conſider the attention at preſent paid to the phænomena of organic nature, we may be ſure that it cannot be long before the reality of ſuch variation is demonſtrated by ſpecific experiments. It muſt be known in all its range before the practitioner of phyſic can be conſidered as an artiſt, guided by fixed principles. And this is but the firſt requiſite. For the nature of the deviations from the healthy conſtitution muſt be aſcertained, before any one can go about to correct the diverſified unhealthy conſtitutions with a ſure hand. Till advances therefore are made in *chemical phyſiology, medical ſcience* muſt continue a chimera.

CASES

Of GONORRHŒA *treated with*

MURIATE OF QUICKSILVER.

CASES of GONORRHŒA

TREATED WITH

MURIATE OF QUICKSILVER.

West Bromwich. 1798.

Dear Sir,

I am ſorry that the reſults of the ſeveral trials which I have made according to your recommendation, of the power of nitric acid in the cure of ſyphilis, have not been ſufficiently preciſe to occupy a place in your intended report on this ſubject. Moſt of the patients to whom I have preſcribed it have made too much difficulty, or have been too irregular in taking the medicine, to afford grounds for any ſatisfactory concluſion, either in favor of its efficacy, or againſt it. At this time, indeed, a venereal patient is employing it with great regularity, and as far as the evidence of a ſingle caſe ſtill under care can be admitted, that evidence is ſtrongly in its behalf. It is a caſe of confirmed lues, of near five months ſtanding, which has reſiſted the operation of mercury, and, together with the remedy perhaps, has extremely reduced the general health of the patient. Both this and the ſpecific diſeaſe are very much altered for the better in the courſe of little more than a fortnight's uſe of the acid, and there is now every reaſon to hope that the caſe will ſpeedily terminate much to the ſatisfaction of the patient, and to the credit of the medicine.

On another ſubject which I mentioned to you when I had lately the pleaſure of ſeeing you at Weſt-bromwich, and on which you did me the honor to requeſt a particular detail of the obſervations I had made, I can now furniſh you with a more preciſe and ſatisfactory ſtatement. I mean the uſe of muriate of quickſilver in the cure of gonorrhœa. By the following caſes copied from memoranda made of them reſpectively as they occurred, you will perceive that in the account I gave you I did not overrate the ſucceſs which has attended this curious, and in the hands of medical practitioners at leaſt, I believe, novel practice. It was introduced to my knowledge by the ſubject of the firſt caſe annexed, and you will not be ſurpriſed, that on the firſt propoſal of the remedy I ſhrunk with fear from the adminiſtration of it. Moſt men, perhaps, in the profeſſion, would have done the ſame. But the urgent deſire of the patient himſelf at length prevailed, if not over my timidity, at leaſt over my reluctance; and indeed I found that whether I was willing to prepare it for him or not, he was reſolved upon taking it; and it muſt be confeſſed, upon no inſufficient grounds, if the account of the perſon who recommended it to him could be at all depended upon: the recommendation was accompanied with an aſſurance that "*it had been adminiſtered to hundreds, and always with ſucceſs.*" This perſon, it ſeems, had been a ſoldier in America during the laſt war, and had there both learned the uſe, and witneſſed extenſively the good effect, of the medicine. With its previous hiſtory I am no farther acquainted. What I have ſince aſcertained will beſt appear in the ſucceeding liſt of caſes. In theſe, ſir, you will ſee that I have occaſionally varied the medicine a little, both in its doſe and mode of compoſition. To this I was led, ſometimes by the fear of irritating too powerfully a conſtitution ſuppoſed to be more feeble and delicate than others, and ſometimes by a wiſh to aſcertain how far the exact formula as given to me in the firſt preſcription, was eſſential to its ſucceſs. The latter appears in the reſults; as to the former, I have to ſay that I have not found in a ſingle inſtance, the ſmalleſt inconvenience ariſing from the exhibition, nor even a painful ſenſation, extending beyond a few hours at moſt of its active operation. In one or two, but I cannot now ſay which of the patients, it produced vomiting; in others only ſome ineffectual efforts to vomit. I have ſtated impartially, I believe correctly, and except in the firſt, as conciſely as I am able, *all* the inſtances in which I have employed the medicine in gonorrhœa.* The concluſions ariſing from the caſes No. IX, and XII, are vague and indeterminate, though it is moſt probable that the medicine ſucceeded in both of them. Nos. III and VI, are perhaps at variance with the reſt. The other nine, I think, afford an evidence no leſs ſatisfactory than ſtriking.

The whole, ſir, are much at your ſervice to be made ſuch uſe of as you ſhall think proper. Your zealous and valuable labours in the promotion and diffuſion of medical ſcience, entitle you at once to the thanks of the whole profeſſion, and of mankind, and to the unreſerved communication of every important fact which may at all coincide with the objects of your more immediate attention.

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* A patient of mine with old and obſtinate ſtrictures in the urethra, and an extremely irritable ſtate of that canal ſo as often to occaſion a long and painful ſpaſm of the ſphincter veſicæ and conſequent ſuppreſſion of the urine, was deſirous of trying its effects in his caſe.—It produced the ſalivation as uſual, but left the diſeaſe unaltered.

I am, Sir,

Your obliged friend and ſervant,

J. ADDINGTON.

CASE I.

JOSEPH M——, aged 35, a healthy mechanic, came under my care on the 14th of Auguſt, with a gonorrhœa which ſoon after its commencement, was marked by the uſual ſymptoms of inflammation of the urethra, and ſwelling of the lips of its orifice, copious diſcharges, heat of urine, chordee, painful erections of the penis, &c. &c. in that degree, which commonly obtains for the diſeaſe the epithet of virulent. The uſual mild, mucilaginous and laxative medicines were preſcribed and regularly taken for ſome time, and in the mean while the man being very deſirous of a ſpeedy cure, was extremely careful to avoid any irregularities in diet or modes of living which might impede his recovery. No material advantage however was obtained by all the care that could be taken. Mild injections, as of tepid milk and water, and very weak ſolutions of lead, and frequent immerſions of the penis in theſe liquids were employed in their turn to no better purpoſe. Every means that could be devised was adopted for rendering the urine mild by larger dilution, and as I ſuſpected that the patient's ſufferings on this ſcore might be augmented by a peculiar diſpoſition to the formation of acid in his ſtomach, affecting him with almoſt continual heartburn, he drank daily a conſiderable quantity of aerated alkaline water, which aſſiſted ſomewhat in the mitigation of them.—But in ſpite of every effort, the diſeaſe continued in a ſtate of painful activity, and after a few weeks, the patient's ſtomach was diſpoſed to reject every medicine. Things were in this ſtate at the end of two months, when the man requeſted my permiſſion to take a medicine which an old acquaintance had recommended to him, and which was ſaid to have cured *hundreds of patients* in this diſeaſe. He was promiſed that it ſhould cure him by taking only *two or three doſes in the courſe of a week or ten days.* Its mode of operation was deſcribed to him in the following manner:—that upon taking it at bed-time, it would in the courſe of a few minutes excite a great burning in his throat and ſtomach, and preſently produce a very copious ſpitting to the quantity as was aſſerted "*of half a chamber-pot full*"—Upon looking at the recipe which he brought me in order that I might prepare it, I was ſtartled, and expreſſed my apprehenſion that it could not be adminiſtered with ſafety; but the eagerneſs of the man to obtain a cure, and his confidence in the report of his friend, prevailed over both his fears and mine, and I made up the medicine according to the following formula:

Take of

Corroſive ſublimate of mercury, 3 grains,
Rectified ſpirit of wine, 1 ounce.—Mix.

Half of this ſolution was to be taken at bed-time. On the morning of the ſecond following day he was ordered to take an ounce of Glauber's ſalts, and in a day or two more to repeat the draughts, and afterwards the ſalts as before; theſe, it was expected, would be ſufficient to complete the cure; if otherwiſe, the medicine was to be repeated at the ſame intervals, with a doſe of the ſalts betwixt every draught, till it was effected.

He took the firſt doſe, viz. one grain and half of the muriate in half an ounce of rectified ſpirit, undiluted, Oct. 14, at going to bed. Its immediate operation on the fauces and ſtomach was exactly what he had been led to expect—a copious ſalivation was quickly raiſed, and laſted from an hour and half to two hours, during which time he ſaid he ſpat more than a quart. The remainder of the night he paſſed as well or better than uſual, and neither then nor afterwards felt any ſort of inconvenience from the medicine. On the next day his complaint was very much relieved. The pain, ſoreneſs, heat of urine, and diſcharge, were all leſſened conſiderably. In the morning following, viz. the 16th, he took the ſalts diſſolved in gruel, and in the evening of the 17th repeated the draughts with the ſame effect as before, and with equal proportionate benefit. The diſeaſe, though not altogether ſubdued by theſe two doſes, was yet ſo much leſſened as to give little or no trouble. However, wiſhing to be thoroughly rid of it as ſoon as poſſible, the man was deſirous of taking on the medicine till it ſhould be perfectly eradicated; and accordingly took four draughts more, viz. on the 21ſt, 26th, 29th, and once afterwards, but on what day I do not know. To me it appeared that the two laſt doſes, if not three, were altogether unneceſſary, the diſeaſe being then reduced to a mere gleet, which from the ſeverity and long continuance of the inflammation might be expected to remain after every active ſymptom had diſappeared: from this time, however, the man remained perfectly well.

CASE II.

ROBERT S —— aged about 40. Gonorrhœa with the uſual ſymptoms, of ſome weeks continuance, but not ſo violent as in the former caſe.

Feb. 28th, 1796. He had two of the draughts and ſalts as preſcribed in caſe No. I, and was directed to take one that evening at bed time, and the other three or four nights afterwards, with the ſalts in the interval. He came again on the 7th of March. The medicine had operated as in the foregoing caſe, and the diſeaſe appeared ſo nearly ſubdued as not to need a repetition of it. The man, however, was very deſirous of two more draughts, which were given to him, with the ſalts as before. This patient did not come to me any more, and ſoon after left the country. But I conſidered his cure as effected in the firſt eight days; which was the more remarkable as he had been under my care for the ſame diſeaſe in the courſe of the preceding year, and was cured in the common way, in the ſpace of about *two months.*

CASE III.

M. C—— aged twenty-two. This gentleman applied to me with what he called the remains of a clap, of which he had ſuppoſed himſelf cured in London, a week or two before, but which he found ſtill exiſting in a ſlight degree. I had no opportunity of examining him, as he thought his own account of the complaint would be ſufficient to direct my preſcription. I ordered him the ſame draughts as in the other caſes, only adding two drachms of water to each. He took the firſt on the night of April 28th 1796, which had no effect whatever either on the fauces, ſalivary glands, or on the diſeaſe. He repeated the doſe on the 30th, and this made no more impreſſion than the former. I had afterwards reaſon to believe that this was a caſe of ſtrictures in the urethra; but as the gentleman left my neighbourhood (being only on a viſit) immediately after it, I did not aſcertain this myſelf with certainty by means of a bougie, but recommended him to do ſo. His gonorrhœa had been treated with injections in London, and I have very often obſerved ſtrictures to follow the uſe of thoſe remedies *in the active ſtages* of this diſeaſe.

CASE IV.

—— SHAW, aged twenty-five. This man alſo I had cured of gonorrhœa in ſeven weeks, in the common way, a few months before. This ſecond infection brought him to me again May 4th 1796. He had two draughts, each containing the ſame quantity, that is, one grain and half of the muriate only, diſſolved in proof ſpirit. On the ninth and tenth he appeared perfectly well.

CASE V.

Mr. J—— aged twenty. This gentleman's was a firſt infection, and extremely virulent.

The inflammatory ſymptoms were ſo urgent as to produce more than once, for ſome hours together, an entire ſuppreſſion of urine. I contented myſelf therefore with employing the uſual means of leſſening inflammation for the firſt week, from June 24th till July 1ſt, and then begun with the muriate. But as his conſtitution was extremely irritable, I gave him at firſt only one grain at a doſe, diſſolved in proof ſpirit. The ſalivation was conſiderable, though not ſo great as in caſe No. I, and the relief of all the ſymptoms in like proportion. The ſame doſe was repeated on the 4th, with ſimilar advantage, though the diſeaſe was not yet ſubdued. Finding this the caſe, and that the patient ſuffered no inconvenience from the medicine, I added half a grain of the ſalt to a draught, which he took on the 7th. The effect of this was greater, both on the fauces and on the urethra; and by three more doſes, viz. on the 11th, 15th, and 27th, the diſeaſe was obliterated, except a gleet, which quickly yielded to an injection.

CASE VI.

JOHN B——, aged thirty-three. March 3d, 1797. Gonorrhœa in the uſual form. This patient took ſix of the draughts, each containing a grain and half, but with leſs effect than uſual; and finding himſelf not perfectly cured, as he had been led to expect, left me, as I ſuppoſe, to apply to ſome other practitioner.

CASE VII.

JOHN H——, aged twenty. Gonorrhœa ten weeks, not violent. Appearing of a weak and irritable habit, I did not venture on the uſual doſe at firſt. Feb. 15th 1797, he took one grain of the muriate, diſſolved in three drachms of ſpirit of wine, and ſix drams of water, with good effect, and no inconvenience. On the 22d, therefore, I gave him a grain and a half in the ſame menſtruum; in a few days a gleet only remained, which was cured by an injection.

CASE VIII.

Mrs. R——, aged twenty four ; a ſoldier's wife : had received a gonorrhæa from her huſband a week or two before ſhe applied to me. It was now in a ſtate of conſiderable activity. Feb. 19, 1797, ſhe took one grain diſſolved in proof ſpirit ; ſalts on the 21ſt, and the ſame draught again on the 22d. Theſe were ſufficient for the cure, which was compleat a few days afterwards.

CASE IX.

Mrs. Z——. I never ſaw this patient, nor knew the progreſs of the diſorder, or the effect of remedies, but by the report of an intermediate perſon. She took four of the draughts, with one grain in each, viz. Oct. 19th, 23d, and Nov. 7th, and 12th ; and afterwards uſed an injection. No farther applications were made to me on her account after the 25th, when I ſuppoſe ſhe was well.

CASE X.

JOHN WALTERS, aged 35. A virulent gonorrhœa, with hernia humoralis. I was called to him on the firſt of January, 1798. I attended to the diſeaſe of the teſtis for the firſt five days, and when this was on the decline, gave him a grain and a half of the muriate, diſſolved in a drachm and a half only of ſpirit of wine, and an ounce and a half of water. The ſalivation took place as uſual, and the diſeaſe was much leſſened by the firſt doſe. The doſe was repeated on the 8th, 10th, and 12th, with ſalts on the intervening days. Both the diſeaſes were now entirely removed, except that enlargement of the teſticle, or rather of the epididymis which always remains a conſiderable time after the activity of the diſeaſe is gone.

CASE XI.

Mr. D——, aged thirty-five. Slight gonorrhœa three weeks. He took a grain and a half

Jan. 5th, 1798, in the ſame menſtruum as the laſt patient. Another doſe on the 8th, put a period to the diſeaſe except a gleet, which as uſual was removed by injection.

CASE XII.

JOHN G——, aged about twenty-five. Jan. 14, 1798. Gonorrhœa in the uſual form, of a fortnight ſtanding. He had two draughts, with a grain and a half in each. I did not ſee him afterwards.

CASE XIII.

DANIEL H——, aged twenty-four. Recent gonorrhœa, with two or three ſmall puſtular eruptions about the ſcrotum and one groin. Having already given the medicine, not only in different doſes, but alſo in different circumſtances of ſolution in the menſtruum, with nearly the ſame effects, I gave to this patient three draughts, each containing a grain and a half of the muriate, but diſſolved in different proportions of the ſpirit and water. That which he was to take firſt had only two drachms of rectified ſpirit, with ſix drachms of water; the next to be taken three days afterwards, had equal parts of them; and the laſt to be taken in three days more, the ſame as the firſt. He was particularly deſired to notice and compare their reſpective operation on his throat, ſalivary glands, ſtomach, and urethra. The firſt draught he thought acted the moſt powerfully in all reſpects; the ſecond and third much alike. By the three, the whole of the diſeaſe was removed, except a very trifling colorleſs gleet, for which when he came laſt I gave him an injection.

NOTE by the EDITOR.

Concerning the ſiphylitic caſe deſcribed in the beginning of the foregoing letter I muſt obſerve that a cure was not effected by the acid.

Mr. Addington doubted at laſt, whether it had diminiſhed the diſcaſe, though it had greatly benefited the conſtitution.—I have heard of the recent employment of large doſes of calomel, alternated with draſtic purgatives in gonorrhœa; and the uſe of corroſive ſublimate, as it is obvious to ſuppoſe, dates from old times, whence it has by tradition deſcended to our own. When the gonorrhœa appeared firſt, or was firſt attended to, as a ſymptom from venereal infection, the ſame remedy would naturally be employed as in other forms of venereal diſeaſe. Girtanner ſays " the internal uſe of corroſive " ſublimate appears to be very ancient. In the " 16th century it was the common remedy of " almoſt all quacks. Phyſicians were as yet " too timid to employ a medicine with whoſe " formidable operation they were acquainted. " Wiſeman is one of the firſt that ſpeaks of the " internal uſe of the ſublimate. He uſes a ſolution in water. Blancard alſo gave ſublimate " internally. Hermann of Leyden recommends " it in gonorrhœa and gave two grains, made " into a pill with extract of liquorice, for a doſe.

" He properly remarks at the ſame time that " this is a remedy only fit for ſtrong conſtitu- " tions. Girtanner's Abhandlung uber die " Veneriſche Krankheit Gottingen 1788. I. " p. 361."

CASE

By *Mr.* KENTISH, *Surgeon,*

NEWCASTLE-UPON-TYNE.

A CASE

OF

MORTIFICATION of the TOES and FOOT.

Mr. C. a Tradeſman in moderate circumſtances, aged 60, in the beginning of November 1796, complained of violent pain in the right leg and foot. He had accuſtomed himſelf to pare off a horny ſubſtance from his heel, which when laſt done he ſaid he had *cut to the quick*. On examining, it was found that the cuticle was off and the cutis dead for the ſize of a ſixpence; and on queſtioning I found they had been uſing all the means recommended by an officious tribe of neighbours to remove this *ſitſaſt* (eſchar.) The part being perfectly dry, the moſt urgent ſymptom appeared to be the pain he ſuffered in his leg. With a view of relieving this ſymptom he was ordered opium in large doſes as recommended by the celebrated *Mr. Pott* ;—alſo to take the bark freely with the intention of keeping up the power of the ſyſtem to prevent further gangrene. The wound itſelf was ſuperficially dreſſed with *ceratum ſaturni*. Theſe means were continued for about a month without any relief—the wound increaſed in ſize and in depth, and now diſcharged a thin, fœtid ichor. The pain ſtill continuing as violent as ever, a fermenting cataplaſm made with infuſion of malt, yeaſt, and oatmeal was applied with a view of correcting the fœtor: This application and the bark and opium were continued for another month. He now began to tire of the bark, and, finding he was not getting better but rather worſe, he took on that peeviſh irratibility ſo unhappy for the individual, and ſo unpleaſant for the medical attendant. One of the toes ſoon afterwards ſpontaneouſly took on the ſame diſeaſed action as had been induced in the heel by cutting away the horny excreſcence ; a ſlight veſicle appeared upon the *top* (I note this becauſe Mr Pott ſays it begins on the inſide in general) of the little toe, without any apparent inflammation. I was not in a hurry to open it, as I did not doubt of the ſtate of the part below; and as it was not inflamed, nor ſo painful as the open wound, it was left to die and dry of its own accord. The wound increaſed in ſize very irregularly. It remained ſtationary on the inſide, and crept along the outſide of the foot as if in order to join its ſphacelated relative, the little toe. When the edges of the wound appeared inflamed, and the ſuppurative proceſs likely to take place, no ſeparation enſued, and the ſurface of the inflamed parts died and came away with the diſcharge, putting on the appearance of a phagedenic ulcer. He had now been attended for upwards of three months, and had taken great quantities of opium and bark in a variety of forms. The applications to the wound had alſo been much varied; emollients, ſtimulants, and antiſeptics, had each been tried without the leaſt apparent benefit, ſo that I own I felt as tired and diſappointed as the patient himſelf. The dread of expence, and the little benefit received, added to the irritable ſtate of mind of the patient, induced me to comply with their implied wiſhes of reſigning him to the care of his *quack friends and their noſtrums* ; ſo that about the middle of February he was left to himſelf, and my attendance ceaſed. Some months after I ſaw his wife, who with an exulting ſmile, told me that her huſband was a great deal better, and that he would be ſoon well now, as they had gotten ſome ſalve which had never failed in *curing ſore legs.* She repeated a number of wonderful cures, with all the embelliſhments of female elocution. To do juſt credit to the inventor's abilities, I congratulated her upon the good fortune of getting ſuch an invaluable noſtrum, and added, with truth, that it would give me great pleaſure if it accompliſhed her wiſhes; ſaying, if they had no objection, I would call in from time to time to obſerve the progreſs of the cure. This they readily conſented to, and I determined to remark the effect of their ſpecific. Accordingly I called, and perceived the diſeaſe had made conſiderable progreſs ſince I had ſeen it; the ulcer had not only reached the little toe, but had crept over the upper part of the foot, near the baſe of the toes. What induced them to think it better was, that ſome parts where the ulcer had travelled, had ſkinned (without any appearance of granulation), and the cicatrix ſhewed the loſs of ſubſtance. I begged them to continue, and joined them in their hopes of ſucceſs. The wonderful ointment they uſed, I found came from Portſmouth Common. A number of extraordinary atteſted cures of men who had been diſcharged from the Royal Naval Hoſpital, and afterwards cured by this ointment, was ſufficient to impoſe upon the weak mind of an invalid, and the ſuperſtition, weakneſs, and ignorance of their neighbouring goſſips. They gave ſome of this ointment, which I found, upon analyſis, was ſomething nearly ſimilar to *ung. reſin. flav.* with the addition of a ſmall quantity of finely levigated *ærugo æris.*

I found the patient's health much in the ſame ſtate as when I ceaſed my viſits, if any thing, rather better than worſe. As the warm ſummer months approached, his pain became more tolerable; he had not taken any medicines ſince I ſaw him, and his diet was not too high or ſtimulating. His appetite was good.

In about a month after I called again, and found the diſeaſe ſtill proceeding. The metatarſal bones were beginning to be expoſed, the ulcer to ſpread upon the ſole, and the phalanges being quite dead and inſenſible, they requeſted me to take them off for them, as that was beyond their ſurgery. Accordingly at different times I took the phalanges off: the diſeaſe ſtill continued to creep on, until the metatarſal bones, and ſome of the bones of the tarſus were much expoſed. A hæmorrhage occurred every now and then, which was ſlight, and ſubſided of itſelf, (N. B. He always felt relief from theſe ſpontaneous hæmorrhages for a few days after they happened). He had now made a trial of the famous means which were to cure him ſo ſpeedily, of ſix months, without finding any benefit. The ceſſation of pain appeared to be more from the warmth of the weather than from any other cauſe; and for the healing appearance of the wound, I apprehend it was more from ſome law of the ſyſtem, of which we are as yet too ignorant to benefit, than from any ſpecific power of the wonder-working ointment. A few cool nights in the beginning of September brought on a return of violent pain, and he now ſeemed willing to loſe his leg, as the only means of ſaving his life, or rendering him free from pain. The ulcer had ſpread to the *malleolus internus*, and the inflammation for two or three inches higher. Both legs were alſo œdematous to above the calf. In conſultation with my friend and partner, Mr. Abbs, he adviſed the limb to be taken off above the knee, for fear the ſame diſpoſition might remain in the ſtump below the knee, as the perioſteum of the *tibia* was already affected. Accordingly on the tenth of September 1797, the operation was performed in the now uſual manner: much of the integuments were ſaved, advanced to cover the ſtump, and put up with ſticking plaſter, with a view of healing by the firſt intention. Having remarked the tendency of every inflammation of the foot to have ended in the death of the part, I wiſhed to avoid exciting the leaſt poſſible unneceſſary action of the parts divided. With this view I uſed nothing but cold water to the face of the ſtump. (I now believe I was wrong in the means, as water of the temperature of the ſtump would have excited leſs than that of a lower temperature, which would favor a re-action.) The femoral artery, at the part where it was tied, was ſtudded with oſſified points, and appeared to have loſt much of its contractility, as it did not retract after the operation, but projected forward like a piece of gum-elaſtic catheter. On examining the amputated limb, the artery was found to have the ſame oſſified appearance. My patient was put to bed and kept quiet; he paſſed a tolerable night; had a conſiderable flow of urine after the operation, and the ſwelling of the remaining foot and ancle had entirely diſappeared before the morning.

He continued quite eaſy, and on the eighth day after the operation, the ſtump was dreſſed.

It appeared well and was ſuppurating kindly, and was now dreſſed every other, and ſometimes every day, according to the quantity of pus that oozed from it.

In about a fortnight all the ligatures came away except the one upon the fermoral artery: at this time every thing looked well. About a week afterwards he had both pain and tenſion of the ſtump, and in a few days an abſceſs formed, which diſcharged itſelf upon preſſure from the dependant external part of the ſtump, which was very much contracted in its dimenſions, ſo as to flatter me with a ſpeedy cure.

The diſcharge continued conſiderable for ſeveral days; it then diminiſhed, and I was in hopes it was done; but on the contrary, in a few days the whole ſtump felt inflamed, attended with ſome ſymptomatic fever. With a view of getting as dependent an opening as poſſible, I applied a cauſtic to the external and inferior part. When the eſchar was cut through a great quantity of very good pus was diſcharged.

This continued, and the whole ſurface of the ſtump was in ſuch an inflamed ſtate as made me dread it would put on the ſame ſphacelating tendency that it had in the foot. Experience in other ſecretions in wounds had convinced me, that to ſuppreſs them, the patient ought not to be excited with a view to his being ſupported under the diſcharge. Beſides, I had my experience of the bad effects that ſtimulants, ſuch as opium and bark, aided by culinary means, and their auxiliaries, port wine, and porter, had had upon my patient during the time of the diſeaſe in the foot.

Feeling myſelf thus intereſted for a favorable termination of the caſe, it made me reflect upon what I imagined to be the cauſe of the original complaint. My patient was about ſixty years of age, had eaten and drank heartily, or in other words, lived well, though not in the higheſt rank of life.

From the diſſection of the ampntated limb, we clearly ſaw the arteries were beginning to oſſify: this diſpoſition I apprehend was owing to their having been too frequently excited by ſtimulant food, and liquor. I do not know any remedy which will diſſolve oſſification of the arteries. I wiſhed my patient to have taken the nitrous acid, with a view of having diſengaged the phoſphoric acid from theſe bony concretions, as I imagined a nitrat of lime would be more ſoluble than a phoſphat of lime, which I ſuppoſe theſe oſſifications are. What would have been the reſult I am unable to ſay, as my patient was much againſt putting himſelf under any regular courſe of medicine.

Having had ſuch ſtrong evidence of great diſeaſe in the blood veſſels, and poſſeſſed with an idea that it aroſe from a too great, and a too long continued excitement, I was forcibly ſtruck with the notion that the beſt mode of relieving the ſyſtem would be, by taking ſome blood from the veſſels. I therefore ordered him to be blooded; and it was not without ſome difficulty I could perſuade them to let my aſſiſtant bleed him. His wife and his neighbours remonſtrated loudly, and ſaid I had more need to give him ſome blood. "What!" ſaid they, "bleed a poor man who has been confined above a year, and is quite reduced to a ſkeleton! oh for ſhame!" However I perſevered, and in a few days he ſubmitted. The blood was very ſizy; eight ounces were taken, and on the following day the appearances were better. He had an eaſier night, and the ſtump was leſs inflamed.

I received great pleaſure from finding the good effects of practice regulated by principle. I therefore determined to purſue the ſame means, at leaſt ſo as to determine, without room for a doubt, on the ſubject. As the ſtump ſtill remained, and the abſceſs diſcharged a good deal of pus, I ordered a dozen leeches to be applied to the inflamed part, and gave a ſmart purge on the following day. This checked the inflammation very conſiderably, and in about a week after they were both repeated. I now found ſo much difference in every appearance, that I was well convinced I was in the right road, and therefore determined to perſevere. As he had for ſuch a length of time been accuſtomed to a drain from his ſyſtem, I ordered him an iſſue in the other leg, and put him upon a bland diet. He was blooded to the extent of eight ounces every week for ſix weeks, and took a purge occaſionally. During this treatment he gained fleſh conſiderably; his ſtump gradually ceaſed to diſcharge, and healed with ſimple ſuperficial dreſſings: and in February, 1798, he was perfectly cured, being a year after his firſt application, the half of which time he treated himſelf. Ever ſince he has remained perfectly well, and has enjoyed a much better ſtate of health than he had previouſly done for years. It is now December 1798; near a year ſince his cure.

As the practice purſued in this caſe was various, and that which appeared to be of the moſt uſe was ſo contrary to what has been recommended by authors, even thoſe of the firſt character, ſome obſervations muſt naturally ariſe from the compariſon. The diſeaſe of my patient was evidently that ſo accurately deſcribed by Mr. Pott in his " Obſervations on the mortification of the toes and feet," and which frequently extends its ravages above the ancle, and generally hurries off the patient in ſpite of every effort of the healing art. Mr. Pott wrote his obſervations with an expreſs view of recommending the uſe of opium in addition to the bark, as may be ſeen from the caſes he relates.* As it is no doubt a very painful diſeaſe, the exhibition of opium, as recommended by Mr. Pott, when it relieves the pain is certainly to be purſued; but there are certain caſes where it does not produce the deſired effect, as in the one I have related. In ſuch caſes, in my opinion, it had better not be continued. The bark was eſteemed an infallible remedy for every ſpecies of gangrene, and was for a long time relied upon and exhibited in all forms, both externally and internally; but as it frequently failed, as it muſt be expected when it was applied to patients placed in the moſt oppoſite extremes of excitement, this in time made its powers doubted, and other remedies ſought for. Mr. White, of Mancheſter, gives ſome obſervations on gangrenes and mortifications, in the 11th vol. of the London Medical Journal, in which he recommends muſk and volatile alkali, and relates ſeveral caſes in which the good effects are evident. It would require a volume to enumerate all the authors who have recommended different means for the treatment of gangrene; and it would only be a waſte of time to mention them and their ſpecifics—I will therefore wave all contradictory opinions of my medical brethren, and hazard one of my own from the appearances that struck me during my attendance upon the caſe related. I have ſeen ſeveral and treated ſome caſes according to Mr. Pott's plan, and muſt confeſs that all I had ſeen had terminated fatally. The want of ſucceſs never ſtruck me to have ariſen from an irrational mode of treatment, but I ſuppoſed it was the nature of the complaint, as both from books and converſation with medical men, I found they were not more ſucceſsful than myſelf. In the beginning of this caſe I ſet off in the old routine, as will be ſeen in the firſt part. When we had proceeded for three months, the patient was fearful of expence, and I was hurt at my want of ſucceſs. This made me beſtow ſome thought upon the diſeaſe, and endeavour to find out a remedy: after the operation, the ſtate of the arteries appeared to me to throw much light upon the nature of the complaint. Although I do not mean to ſay that in all ſuch caſes the arteries are oſſified, yet as the cauſe of the diſeaſe in this inſtance could be fairly traced as produced by that effect, according to the rules of ſound logic, no further cauſes ought to be admitted; and though in ſome caſes oſſification may not have taken place, yet the diſpoſition to take on that proceſs may of itſelf produce the effect on the extremities.*

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* If the patient have been accuſtomed to great excitement by fermented liquors, the uſe of opium as a different ſtimulant, with a view of leſſening the former one, may be a highly uſeful means. But it is too frequently the caſe in the practice I have ſeen, that when opium and bark are given, all the other high range of ſtimuli are conjoined—ſuch as wine, brandy, volatile alkali, and above all, the richeſt or ſtrongeſt animal diet with ſpices, &c.

It appears to me that the diurnal uſe of fermented liquors and good animal food for a number of years, had induced ſuch increaſed action of the heart and arteries as might, for a long time, have appeared as increaſed ſtrength. We know it is a law of the animal economy, that if any muſcle is much uſed it enlarges, and, to a certain point, becomes ſtronger, yet if this very ſtrengthening proceſs be carrried to too great a length, it becomes the cauſe of exhauſting that very power which it before appeared to give.§ Thus do I ſuppoſe it to have been with the heart and arteries of my patient: the exceſs of ſtrength in the veſſels ended in their weakneſs. The neceſſary conſequence of exciting the ſtomach to greater action is alſo an increaſed action of the heart and arteries; and if we ſee the larger veſſels increaſed in action, we muſt take it for granted that the whole ſeries will ſympathize; if ſo, the ſurface veſſels of the whole ſyſtem muſt feel the effect. As it is with increaſed action, ſo it is with diminiſhed action: when the ſtomach is torpid, ſo it is with the heart and arteries, and all the ſurfaces of the body to a certain degree.* If the ſtomach be rapidly excited by ſpirituous liquors, the head is moſt frequently the part which ſympathizes, if leſs rapidly the liver appears affected; but if by gentle, gradual, daily excitement, the feet ſympathize in what is termed gout, and ſometimes in the gangrenous ſtate of the toes and feet which the above caſe has related.

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* Old people are ſubject to oſſifications of the heart and larger arteries; and who can doubt but by too great excitement we bring on the appearances of old age, when from number of years it ought to be at ſome diſtance.

§ Hard-working people in general, although they appear and are for a certain time ſtronger, very ſoon wear out; i. e. they bring on premature old age. Perhaps it will be found to be premature old age whether the ſyſtem be too much excited by the too great exertions of the body from the ſtimulus of want, or the too great excitement of the ſtomach by indulging in profuſion.

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* Every practitioner muſt have had various opportunities of ſeeing this in gouty attacks of the ſtomach: the coldneſs of the extremities, the weak and fluttering pulſe, cold ſweats, and ſometimes the relaxation of the ſphincter muſcles, cauſe trains of ſymptoms which can only be obviated by the prompt interference of a bold practice, founded on the knowledge of the laws of the living ſyſtem.

There are various ſurfaces capable of this ſympathy, which we may mark every day in gouty habits, but all I believe proceeding from the ſame cauſe. It will be aſked, would I pretend to cure gout by bleeding? to this I ſhall anſwer, that if my patient were in the torpid ſtate, I certainly ſhould not; as I am well aware that in that ſtate even ſtronger ſtimulants than what had brought him to that point might be neceſſary, to prevent him from finiſhing in the laſt ſtate of torpor, viz. death. Yet I muſt here remark, that to relieve is not to cure. Having relieved him by ſtimulants, to cure him even of the gout I would both bleed and purge, and make him leſſen his diet gradually, ſo that his ſyſtem might have time to recover that power which he had before incautiouſly waſted.*

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* I muſt not be underſtood to aſſert that all caſes of the gout would be relieved by bleeding and purging—No! I

With ſuch views, and by ſuch means have I cured my patient. I have had ſome other caſes where the circumſtances have been different, but the cauſes the ſame, and, by treating them upon the ſame principle, the complaints have yielded in a much ſpeedier manner than when I have treated them in the routine—according to the nomenclature of noſologiſts.

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am too well aware of the proteus-like ſymptoms of that complaint, and that we muſt act in many inſtances according to exiſting circumſtances. In all gouty caſes in which bleeding and purging will be of *more uſe* than any other means.

OBSERVATIONS

ON

CARBUNCLE,

BY

MR. YONGE, SURGEON,

SHIFNAL, SHROPSHIRE.

To Dr. BEDDOES.

Dear Sir,

I wiſh you may think the following caſe intereſting enough to deſerve admiſſion amongſt your medical contributions.—The report is condenſed as much as ſeems proper, and the few annexed obſervations will help to explain, if not to recommend the treatment.

Conſidering how long the beneficial influence of cold in variolous fever has been known, I am at a loſs to account for the ſlow production of thoſe important conſequences which this diſcovery has furniſhed. Dr. Currie's intereſting remarks upon the agency of heat and cold, have introduced the ſubject to more extenſive enquiry and experiment; they recalled our old and frequent converſations upon the effects of cold in various diſeaſes; and the caſe of your little pneumonic patient, Miſs Browne, where cold ablution ſo happily verified your prediction of its efficacy, and the great ſtreſs you laid in theſe diſeaſes upon cold air.

In Savery's letters from Egypt, is mentioned a curious inſtance of the advantage of cold expoſure in arreſting the ſymptoms of plague; but amongſt many public medical letters and reports which I have examined in the Philadelphia papers, I do not find any practical inference from the repeated ſuppreſſion of their fever by the approach of winter, and I know only of one phyſician (Dr. Maclean) who tried the affuſion of cold water in the Weſt-Indies.*

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* A phyſician to the army told the editor that during the flight of the Britiſh troops from the banks of the Waal to Embden (during that very ſevere weather when the French conquered Holland) the fever patients, who were much expoſed to the froſt in conſequence of the mode of conveyance, and the precipitate movements of the army, all recovered. It is to be hoped that the details of this event will be placed on record. See the annexed remarks.
Editor.

As a topical remedy in inflammations, cold was long almoſt excluſively employed to mitigate the pain ariſing from recent ſcalds and burns: it has of late been more generally introduced in the treatment of other local affections; but I have reaſon to believe its beneficial effect in carbuncle not generally known, though ten years ago Dr. Withering preſcribed it for a lady whoſe caſe is hereafter noticed. I have ſince had another opportunity (beſides that offered by the caſe particularly related), of confirming the ſucceſs of this practice, under which the patient recovered in his ſeventieth year; neither of theſe carbuncles were ſo extenſive as in the preſent inſtance, but the efficacy of the application was in all equally remarkable, and I think deſerving of record.

I am, dear ſir,

ſincerely yours, &c.

To
DR. BEDDOES.

W. YONGE.

SHIFNAL, Dec. 20, 1798.

CASE OF CARBUNCLE.

The ſubject of the following caſe is forty years of age, and of robuſt make and conſtitution: long accuſtomed to great and continued action of body and mind, and to liberal diet; he had experienced during many years, occaſional indiſpoſitions from various inflammatory affections; ſuch as rheumatic, or irregular gouty pains, (for more than one judicious phyſician doubted about their nature), uneaſy ſenſations about the ſtomach and liver, often accompanied with febrile ſymptoms. He was alſo ſubject to gutta roſea, and to very irregular action of the heart. In the ſummer of 1795, after many hours of exertion and anxiety, half immerſed in water, and expoſed to heat, he was attacked in the night with ſevere pain and inflammation of the great toe, which continued during two or three days, with all the ſigns of perfect gout.

Symptoms of general inflammation then ſupervened, and the diſeaſe then aſſumed the character of rheumatiſm, from which he recovered with much hazard and difficulty. From that period his pulſe became, and has continued, perfectly regular; but rather feeble, and ſeldom leſs than 86—90 per minute. His other habitudes remained nearly as before, and during the laſt two years the equinoctial periods have been more diſtinctly marked by gouty and other ſymptoms, denoting conſiderable excitement of the ſyſtem.

Under circumſtances not very diſſimilar from thoſe preceding his former illneſs, a pimple aroſe near the middle of his back, in appearance like a ſmall dark-colored variolous-puſtule, very painful, hot, and exceſſively ſenſible to the touch; its baſe inflamed, of a deep ſcarlet redneſs, about an inch in diameter, and the ſurrounding integuments indurated to the diſtance of two inches. Pulſe in the evening, 90, in other reſpects well.—Six grains of calomel at bedtime, and a poultice of bread and milk to the affected part.

Auguſt 5th. Patient ſlept little; pain ſevere; diſeaſe rather increaſed; has had copious diſcharge of very bilious ſtools; pulſe 98; four grains of calomel, and one of opium in the evening. Poultice continued.

6th. Pain much increaſed; hardneſs and inflammation but little extended; no ſleep; has had two evacuations ſimilar to thoſe of yeſterday; pulſe 106 in the evening, but not full nor hard; compreſſes of fine linnen dipped in cold water applied over the part, and very frequently renewed. Opium and calomel two grains of each at bed time.

7th. Cold applications relieve the pain and keep the patient tolerably eaſy; had ſome ſleep; inflammation extends; the color deeper, and hardneſs increaſes; no ſwelling of the integuments, nor any ſigns of ſuppuration; pulſe 108 in the morning; urine natural; has had one very large bilious ſtool. Two grains of opium at bed-time; takes ſaline julep; gruel, and toaſt and water freely.

8th. Patient ſlept little, partly owing to the cold applications, which are almoſt continually renewed, to keep him tolerably eaſy; pulſe 120; inflammation extends rapidly; very dark colored; about four inches in diameter, with ſmall veſicles on its ſurface; bled to 10 ounces. Six grains of calomel this morning have produced three very copious and very fœtid diſcharges of bile. Opium, ſaline medicine, and cold water continued.

9th. Quiet ſleep, and freedom from much pain; inflammation extends, but better defined in its circumference; blood taken yeſterday inflamed; heat and induration very great, and no ſigns of diſtinct tumor; one drop of healthy pus from the pimple this evening; two very bilious evacuations; urine high colored; pulſe 110. Opium and cold applications continued.

10th. Patient ſleeps quietly at intervals, night and day, as the applications and conſequent eaſe enable him to do; takes mild food well; pulſe 108; one very copious diſcharge of bile; good pus continues, now and then a drop; inflamed part about ſix inches in diameter; ſenſibility of the ſurface diminiſhed; color near the centre almoſt livid: heat and hardneſs exceſſive; opium two grains night and morning; cold water continued.

11th. A very reſtleſs night; general heat and thirſt; pulſe feeble, 124; inflammation extends; heat of the part feels intenſe, and the patient only kept eaſy by almoſt unremitting application of cold water; diſcharge increaſes, a drop of good pus appearing on almoſt every compreſs—bled to eight ounces—four grains of calomel have produced three bilious ſtools; feet and ancles ſwelled in the evening; takes mild food plentifully; opium and cold water continued.

12th. Slept well at intervals; heat and thirſt abated; pulſe 110; blood inflamed; local diſeaſe extends ſlowly, but ſome projection of the inflamed part above the ſurrounding integuments, and near the centre is a little ſoftened. One bilious and very fœtid evacuation; healthy pus diſcharged more freely. Opium and and cold water continued.

13th. Patient ſleeps better; pain diminiſhed, and good pus flows abundantly from the opening puſtule; pulſe 104; ſwelling of the legs and feet increaſes; inflammation does not extend, and integuments riſe. Two grains of calomel has produced one bilious evacuation; opium and cold water continued.

14th, 15th. Opening expands, and diſcharge of good pus very great; tumor well defined; heart ſhaped, with the apex upwards, and meaſuring ten inches in its tranſverſe diameter; projection nearly equal over the whole ſurface, but hardly one inch in any part; of a dark mahogany color, and ſeems livid towards the center; ſurrounded by a ſcarlet zone, frequently varying in extent and intenſity. Heat continues very great, and pain is only ſuppreſſed by cold; appetite tolerable, and ſleep better; but the patient is much emaciated, his ſtrength impaired, and the oedematous ſwelling of the legs increaſes—opium at bed-time, and cold water continued.

16th. This morning, enlarged the natural opening in a perpendicular direction, to near four inches. Wound bled freely; dreſſed ſuperficially, and the whole tumor covered with pledgets of ſaturnine cerate—*cold water diſcontinued.* Patient in the evening complains of exceſſive pain—inflammation increaſing rapidly, and heat intenſe—cold compreſſes re-applied, and in half an hour the patient is eaſy. Opium, two grains at bed-time; peruvian bark and aromatics every four hours.

17th. 18th. 19th. Diſeaſe varying frequently in its aſpect: the ſcarlet zone alternately expanding and contracting. Cold applications at no time more remarkable in aſſuaging pain and reſtraining inflammation—diſcharge of good pus very profuſe—bowels regular, but ſtools continue bilious—pulſe 108--116—patient ſleeps well, but his ſtrength continues to decline—his appetite is impaired—the ſwelling of his legs increaſes, and he ſweats profuſely—continues opium, bark, aromatics and cold water.

20th. Tumor ſoftened and feeling ſpongy in its whole extent—two lateral openings made, beginning near the upper part, about an inch on each ſide the central inciſion, and carried, diagonally outwards, to the moſt depending parts of the tumor and through the ſubſtance of the diſeaſed cellular maſs—dreſſings of the mildeſt kinds, the whole covered as on the 16th, and water diſcontinued.

21. Recurrence of pain again induced the application of wet compreſſes, which relieved the patient as uſual—diſcharge of pus very profuſe—tumor diminiſhes and colour changing to a duſky brown—continues opium, bark, &c. and cold water over the dreſſings.

22. 23. Patient ſleeps much, appetite improves and oedematous ſwellings diminiſh—

wounds look healthy, and membranous ſloughs begin to ſeparate—bowels regular, and ſtools leſs bilious—ſweats continue—bark with the addition of vitriolic acid, and cold applications continued.

24. 25. Patient ſleeps much—diſcharge leſs, and ſloughs ſeparate—inflammation nearly gone, and integuments begin to adhere—cold water diſcontinued, and bandage applied—ſweats continue—two ſmall pimples near the inferior edge of the tumor, inflamed and ſore, reſembling the incipient carbuncle.

26. 27. Suppuration and union proceed favourably, and the patient improves rapidly, but freſh puſtules appear.

28. 29. Puſtules increaſe in ſize, with pain, heat, and inflammation, having all the characters of ſmall carbuncles—patient feveriſh—pulſe 110, bark and aromatics diſcontinued—opium and calomel of each one grain every twelve hours, and the bowels occaſionally excited by ſmall

doſes of rhubarb or natron vitriolatum—cold compreſſes to the painful tumors.

30. 31. Puſtules leſs painful and ſuppurate ſucceſſively, but new ones appear below—wounds healthy and union proceeds favourably. Patient improves in appetite, fleſh, and ſtrength; but ſweats continue.

Sept. 1ſt, 2d, 3d. Union of integuments rapid, but the minor carbuncles exceedingly troubleſome, and increaſe in number. Cold compreſſes and ſaturnine applications, as they ſeem reſpectively neceſſary; the ſkin being frequently ſmeared with olive oil.

4th, 5th. Freſh puſtules ſpreading downwards, and the former ones ſucceſſively ſuppurating, with the diſcharge of ſmall membranous ſloughs—cold water ſeems to keep them eaſy, but its effect leſs remarkable than in the firſt inſtance—wounds incarn favorably.

6th. One of the tumors has within a very few hours increaſed to five inches in diameter—ſwelling and hardneſs of the integuments conſiderable, but leſs heat and inflammation than in the firſt carbuncle. Pain ſevere; pulſe 120. Six grains of calomel produced three copious evacuations—cold compreſſes applied, and renewed as at firſt.

7th. Diſcharge of ſanious matter from the tumor, attended with *exceſſive* pain during a minute; ſubſides faſt; pulſe 106. Small tumors continue to ſpread and ſuppurate, the whole aſſemblage creating great diſtreſs and inconvenience to the patient, who nevertheleſs improves in his fleſh and ſtrength; ſwelling of the feet gone; ſweats continue. Opium and calomel one grain every night, with moſch and valerian in larger doſes three times a day. Cold water continued.

8th, 9th. New carbuncle reduced to its original ſize; patient in other reſpects as yeſterday.

It is unneceſſary to continue the diurnal report of this caſe. The ſmall tumors continued to ſpread downwards over the loins, hips, and nates, ſuppurating ſucceſſively, and varying in ſize from half an inch, to an inch and half in diameter. Neither roſe water nor ſaturnine lotions were more efficacious than common water, to correct the occaſional pain and exceſs of inflammation, nor did any applications check their production, which ceaſed not much before the final incarnation of the wounds, about the end of October. Moſch, valerian, peruvian bark, aromatics, and opium, were alternately employed as circumſtances directed. In the middle of October, the patient became rather ſuddenly conſcious of great diminution of ſtrength, obſerving at the ſame time an apparent want of bile in the alvine evacuations, which continued of a whitiſh or light aſh color during a fortnight, but without any change in the color of his ſkin or urine. He then diſcontinued all other medicines, and after taking a few grains of calomel, the ſtools reſumed their natural color, his ſtrength returned, and he at preſent enjoys better health than uſual.

Medical writers have diſtinguiſhed two ſpecies of carbuncle by the terms *ſimple* and *malignant*; the laſt ſignifying thoſe tumors attendant upon the plague, and on ſimilar fevers; to the firſt kind only the following remarks are applicable.

The deſcription of this diſeaſe uſually found in books, impreſſes an idea of its neceſſary connection with a putrid ſtate of the ſyſtem; and the aſpect of the inflammation helps to confirm this prejudice. In the true *authrax gangrenoſa*, mortification occurs amongſt the firſt ſymptoms, or ſucceeds ſo rapidly as to preclude all hope of relief. But this is a very rare diſeaſe, and we may ſafely affirm, that in a great majority of caſes, ſphacelation is preceded by intenſe inflammation, and generally is the conſequence of it. In the preſent inſtance, the great change of color that took place on the 7th, 8th Auguſt without any appearance of ſwelling, or other ſign of ſuppuration, gave reaſon to fear that ſome deſtruction of the integuments muſt enſue from ſphacelation; and I believe this would have been the inevitable conſequence of any cordial antiputreſcent plan adopted to prevent it. But though exceſs of inflammation conſtitutes probably the moſt frequent ſource of danger, another ſtill greater may reſult from debility, and deficiency of living power, as ſometimes happens in aged and infirm patients; the circumſtances of each individual caſe muſt therefore determine where, and to what extent, evacuants ought to be employed. Of theſe, bleeding, the moſt obvious and certain, is not frequently uſed, owing chiefly to the prejudice before mentioned. On the cure of *anthrax ſimplex*, Sauvages obſerves, "*raro phlebotomia indicatur aſt a vigore pulſus in hac ſpecie non nocet*;" but the late Mr. Bromfeild, one of the beſt modern writers on this diſeaſe, decidedly recommends bleeding, even under a languid ſtate of the pulſe, if no other ſymptoms of debility correſpond to forbid it.

I have lately been informed by Dr. R. Darwin, of the unexpected recovery of an old perſon from bad carbuncle, after dangerous hemorrhage from the noſe; and now am inclined to believe that veneſection at an earlier period of the preſent diſeaſe, might have been attended with advantage.

The exceſſive ſenſibility of the part of the deep ſeat of the diſeaſe, and the little chance of ſucceſs from ſimple reſolution, may perhaps be objected to topical bleeding. I have no experience of its efficacy, nor find it recommended; but have heard that free inciſion through the whole extent of the indurated parts, at a very early period, has changed the character, and ſtopped the progreſs of carbuncle.

The cellular or adipoſe membrane is the primary ſeat of this diſeaſe, which is quickly communicated to the ſkin. Linnæus defines it, "*glandula ſubcutanea inflammata in ſuppurationem malignam vergens.*" The acknowledged efficacy of mercury in *variola* and ſome other inflammations, may alone recommend this evacuant in ſimilar caſes; but experience of the beneficial influence of calomel in habitual indiſpoſitions of this patient, confirmed thoſe motives of preference which looſer analogies ſuggeſted; and one circumſtance particularly induced perſeverance in the uſe of this remedy. Dr. Darwin remarks that gout and rheumatiſm are not primary diſeaſes; but ariſing from a transferred morbid action of ſome other part of the ſyſtem, and generally of the liver; no previous indication either of torpor or increaſed action of that viſcus was obſerved; but the patient had been ſubject to hepatic affections; and the copious diſcharge of bile throughout the whole inflammatory period, ſeemed to indicate a connection, which the ſubſequent deficiency or change of ſecretion confirmed.

A lady about forty years of age, and very liable to inflammatory affections, was attacked with pain and ſoreneſs in the region of the liver, feveriſh ſymptoms, and diſcharge of bile from the ſtomach and bowels. She was repeatedly bled; the ſpontaneous evacuations were encouraged, and ſuch additional remedies employed as promiſed to relieve her; after continuing ten days in this ſtate, a carbuncle appeared between her ſhoulders; the bilious ſymptoms then ſoon ſubſided, and with the application of cold water, ſhe recovered after a few weeks confinement. May we infer from hence that anthrax, like gout, gutta roſea, urticaria, &c. is ſometimes the offspring of hepatic diſeaſe?

Little remains to be obſerved relative to the medical part of the report. A phyſician, whoſe candor and judgment confirmed us in the general plan, adviſed muſk and valerian as ſubſtitutes for peruvian bark and aromatics, with a view to ſupport the ſtrength by a new ſtimulus, as the uſual ones were withdrawn, and from their repeated uſe in mortification of the extremities. A remedy ſo powerful as opium, and whoſe effects are ſo well known, needs no recommendation; and it is hardly neceſſary to add, that the dietetic regimen correſponded with the variation of the medical treatment.

The topical remedies recommended in carbuncle, are ſuch as might be expected from the gradations of its appearance and character; varying between the extremes of actual or potential cautery; and emollient poultices and fomentations. The ſelection and adaptation of theſe muſt be left to the judgment of the practitioner, and to the exigency of the caſe. The preſcription of a general rule may perhaps be ſafely admitted, that whatever application aggravates pain, is improper, and this will ſufficiently guard us againſt the abuſe of ſuch ſtimulants, as under the notion of preventing or impeding mortification have been too frequently employed. It has been already obſerved that *exceſſive* inflammation uſually precedes deſtruction of the diſeaſed parts, and in eight caſes of carbuncle which have fallen under my notice, this was invariably true. *Pain*, *heat*, and *rubeſcence*, determine the degree of this exceſs; and whatever is found moſt effectually to ſuppreſs theſe, without danger to the life of the part, will be the beſt remedy in that particular caſe. The ſuperior advantage of cold applications, and in what degree they may be ſafely and moſt ſucceſsfully uſed, farther experience muſt decide. Water ſeems only preferable to other fluids, as an inert medium, eaſily procured; which tranſmits heat, and evaporates readily. I have not tried it in any caſe of extenſive inflammation colder than forty-five degrees of farenheit's thermometer.

The moſt immediate and obvious effect is diminution or ſuſpenſion of pain. A cambric handkerchief, folded into ſix or eight doubles, and dipped in ſpring water, was applied over the inflamed part, and renewed as the ſenſations of the patient determined, at intervals; varying at different periods of the diſeaſe, from five minutes to half an hour. Sometimes the water was poured in a quick ſucceſſion of drops, or in a capillary ſtream, over the ſurface of the inflamed part, and from this mode he ſeemed to derive the moſt perfect exemption from pain.

On the 11th Auguſt, the heat of the integuments was 106°, but its rapid evolution and diffuſion, impreſſed the ſenſe of a much higher degree. The hand, when ſwept gently over at the diſtance of three or four inches from the ſurface, received a ſenſation ſimilar to that which a heated andiron would have given at the ſame diſtance, and water by flowing over the tumor in drops, as before deſcribed, acquired fifteen degrees of heat. The compreſſes were warmed ſo much as to be no longer uſeful in four or five minutes, and if by accident the regular ſucceſſion of theſe was interrupted, pain, heat, and redneſs, certainly increaſed. Sometimes the applications were unremittingly renewed during a certain time, i. e. till the pain had nearly ceaſed; and this point ſeemed to limit their efficacy, for beyond it, cold produced an uneaſy ſenſation, which always induced the patient to ſuſpend the proceeding. The utility of cold was particularly evident—1ſt. Upon the 6th of Auguſt, when ſubſtituted for the poultices—2d. Upon the 16th and 20th after being diſcontinued for a ſhort time, and 3d. upon the 6th and 7th of September, in arreſting the progreſs of the new carbuncle; but during ſix weeks, very few days elapſed without ſome deciſive proofs of its efficacy.

The ſecondary tumors or buboes are uſual conſequences of carbuncle.—Mr. Bromfeild aſcribes them to injury of the membranes, which he obſerves are often deſtroyed beyond the extent of the inflamed integuments; admitting this explanation, it may be aſked why theſe puſtules are not diſtributed more equally around the circumference of the tumour but ariſe in regular progreſſive deſcent as before deſcribed? has not abſorption of the pus from the ſurface of the back, down which it flows, or the gradual diffuſion of it through the cells of the ſubjacent membrane ſome influence in their production? however this may be, I am ſorry to add, that none of the various means uſed, ſeemed to prevent theſe minor carbuncles, which ultimately proved hardly leſs troubleſome than their principal.

One circumſtance more alone ſeems to merit attention; viz: the advantage of a decumbent poſture in every period of the diſeaſe, owing probably to muſcular relaxation, but the patient, whoſe uncommon fortitude enabled him generally to regulate the meaſures by his own experience, could not fully avail himſelf of this obſervation.

ADDITION BY THE EDITOR.

A ſingle glance will nearly ſuffice to ſatisfy the philoſophical obſerver that there has been long wanting to medicine a great body of information *concerning the treatment of diſeaſes by temperature*. Convinced of the practicability of ſupplying this deſideratum, many years ago I preſſed different medical friends to avail themſelves of various well-known analogies.

In the above-mentioned caſe of the perſon of 70 years of age, the application of cold was, in part at leaſt, occaſioned by ſuch a converſation. And I well remember hearing daily of the happy effect of the expedient. When it was neglected by the attendants (as ſometimes happened,) the pain and inflammation returned with their original violence. Hence the patient's friends were rendered doubly diligent: and they had ſoon to congratulate him upon his eſcape from a danger, from which at that age no other plan would probably have reſcued him; not to mention the dreadful pain, preceding mortification, of which he endured but little.

The idea of regulating extenſive as well as limited, morbid actions, by temperature, (though never purſued as far as it ought to be) has ſurely occurred to thouſands of practitioners. In the particular caſe of carbuncle, cold applications have been ſuggeſted; among others, if I miſtake not, by Mr. Rigby in his publication on animal heat. Still however this practice undoubtedly requires to be enforced. In Ploucquet's laborious digeſt of medical facts, *Initia bibilothecae medico-practicae, et chirurgicae realis,* the method is not even hinted, under the heads *anthrax*, *carbo*, *carbunculus*, *ignis perſicus*, *pruna*.

Nay more; I have good cauſe to know that there are in this country ſituations, where a dangerous attack of carbuncle would place the ſurgeon between the alternatives of hurtful routine practice at the hazard of the patient's life, or of rational practice at the hazard of his own reputation. Among the correſpondents whom I have induced to apply cold water or ice during the inflammatory ſtage, the very caſe has occurred. From one of theſe I have a letter before me, admirably delineating at a ſingle ſtroke that claſs of medical men, who having acquired vogue by qualities entirely foreign to their profeſſion, preſs with the whole weight of their authority upon the art itſelf, leſt it ſhould advance and leave them behind.

The patient mentioned by my correſpondent, had received almoſt entire caſe from cold water; and the inflammation ſeemed to be held in check, when from the anxiety of friends, another (faſhionable practitioner) was called in. "His diſapprobation of our practice," ſays the account, "was immediately manifeſted by the "preference he expreſſed for the old plan of "treatment by emollient applications and poul-"tices; *mingled with affected admiration of the "courage which had puſhed to ſuch an extent, the "trial of a new remedy.*"*

From part of the following ſentence may one not ſuppoſe that the eye of a keen obſerver had been caught by thoſe members of the medical fraternity that without reflection or remorſe, go on eternally repeating the ſame vain proceſſes of their art; and never let ſlip an occaſion of blaſting by inſinuation the character of a rival, who to aſſuage pain, or preſerve life, ſhall dare depart from precedent? Eſt-il une fin plus triſte (ſays Rouſſeau, lettre à M. de Voltaire 18 Août 1756), *que celle d'un mourant.... que les medecins aſſaſſinent dans ſon lit à leur aiſe,* et que des prêtres barbares font avec art ſavourer la mort?

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* I ought to add, that the diſcontinuance of the compreſſes wetted with cold water, in the caſe before related at length, aroſe from a ſcruple expreſſed by a ſenior ſurgeon of great reſpectability; ſo little is the method underſtood. This circumſtance has been ſuppreſſed by the reporter out of delicacy; but it ought to be mentioned; otherwiſe the treatment would appear capricious. I need not ſay how ſtrongly the variation confirms the doctrine deducible from the caſe.

Leſt the foregoing facts or my remarks ſhould miſlead, it ſhould be added, that free application of cold ſeems only admiſſible in genuine primary inflammations, threatening to end in ſuppuration, extreme debility, or gangrene. In ſecondary affections, not attended by ſtrong pulſe, and exiſting in feeble conſtitutions, ſuch treatment ſhould be cautiouſly purſued. The following caſe will illuſtrate the rule.

A lady had ſwelling, pain, and heat, in one cheek immediately after tooth-ache. On the ſecond day no abatement. I adviſed her to put wet linnen to the cheek. No effect followed in a quarter of an hour. The pulſe being weak and not frequent, I did not puſh the cold application. Next day, towards evening, the pain and heat were intenſe; the ſwelling larger, with redneſs. The throat was a little ſore. At intervals a pain was felt to dart from the bottom of the ſternum to the ſpine. The pulſe was not ſtronger or more frequent than in health.

Four folds of filtering paper, dipped in pump-water mixed with a little ſpirit, were laid on the cheek, and very frequently renewed for above half an hour. At firſt ſome eaſe ſeemed to follow. But the patient ſoon began to complain of an increaſe of the darting pain; and at laſt diſtinctly obſerved for a number of times in ſucceſſion that every renewal of the cold application brought it on with violence. At the ſame time ſhe remarked that the pit of the ſtomach was drawn inwards. Hence (the pulſe being under 80, and weak) I judged the darting pain in the cheſt to ariſe from irregular contraction of the diaphragm, and not from inflammation: and as this pain had now become exceſſive, ſuperſeding all ſenſation from the check, I deſired that the cold application might be diſcontinued, and a grain of opium taken. This was done, and the pain in the cheſt ſoon ſubſided. The patient ſlept well (which had not been the caſe for two nights before) and found the cheek much reduced in the morning.

We often hear of the danger of expoſure in the meaſles. I knew an inſtance where a delicate woman, towards the cloſe of the complaint, had ſpaſmodic ſeizures on the admiſſion of cold air into her apartment. I ſuppoſe, in conformity to the rule above laid down, that this happened from inattention to the ſtage of the diſorder, and that it would not have happened during the inflammatory period, while the diſeaſed actions were ſtrong.

The caſe of Miſs Brown, with others of the ſame kind, will hardly be thought remarkable ſince Dr. Currie's able treatiſe on cold ablution in fever. It was briefly this. She had ſeveral days before I ſaw her been affected with pneumonia. Her ſtrength was ſo reduced as to raiſe apprehenſions (to which I ſhould not probably now yield) reſpecting further evacuation. I then recommended only that ſhe ſhould be kept in a room without a fire, and that a mixture of water with ſpirit ſhould be frequently applied to the trunk; the extremities being covered ſufficiently not to feel cold. The ſucceeding day the heat of the ſkin, and the diſtreſs of reſpiration were removed, and ſhe grew gradually well.

The following caſe ſhews the danger of inadvertence in not keeping up attention to temperature, and would have appeared to the late Dr. Brown a valuable fact. Maſter Y. four years old. Dec. 29, 1793; has a dry hard cough—ſome pain in his cheſt; pulſe 140. Face fluſhed—ſkin exceedingly hot—tongue covered with a thick white cruſt—eyes very heavy—eyelids puffed. Had an antimonial emetic laſt night.

As his cheſt is very narrow, and his habit feeble, bleeding is ſtrongly oppoſed, though it ought undoubtedly to be practiſed.

℞ Miſtur. ſalinos. ℥i
Antimon. tartarizat. gr. ¼ M Capt.
ſecundâ quâque horâ.

At night rather better. 30th in the morning, ſymptoms little abated: medicine continued—ordered to be kept in a room without fire; but the extremities not to be ſuffered to grow chill. At night breathing eaſier, cough leſs; ſkin cooler—pulſe only 92: two looſe ſtools. Diſcontinue the medicine. Dec. 31ſt. Had ſlept with heavy bed clothes—towards morning appears very ill. At eight o'clock pulſe 140, weak. Cough frequent, with dyſpnœa, but looſer, with ſecretion of mucus which he does not ſpit out. Tongue foul, browner. Qu. Is not the cauſe of this violent exacerbation his not being kept cool in the night as well as the day? Directed to be taken out of bed and kept cool. Eleven o'clock

A. M. Pulſe has fallen to 120, and is ſtronger—leſs cough—no laborious reſpiration—no pain in the ſide—ſkin cool—no appetite. One o'clock P. M. Had riſen. Pulſe 108—ſkin cooler—tongue moiſter. Eight o'clock. Sleeping *upon* the bed—ſleep compoſed—body of a natural temperature to the touch. A glyſter had been injected, and had opened the body. To night to have leſs bed-clothes, and to be ſtripped or taken up if he grows hot. Jan. 1ſt. Has had a good night—appetite better—cough looſe. From this time he amended, and by degrees perfectly recovered.

To Dr. Hamilton of Ipſwich who, under the diſadvantage of blindneſs, has improved his treatiſe on hydrophobia into the beſt repoſitory of facts extant upon this ſubject *(Remarks on hydrophobia. Longman.* 1798*)*, I am indebted for a very intereſting fact concerning the operation of cold in inflammatory affections of the cheſt.

Inſtance of CATARRH *cured by expoſure to open air in a froſty night.*

The following anecdote may be worth your notice. It appeared to me ſo uncommon that I had it written down at the time it happened which was on the night of the 27th of February 1797, and ſucceeding morning. Our weather, from the beginning of Feb. was extremely variable as to heat and cold, ever running into extremes for the ſeaſon, but ſteady with reſpect to the abſence of rain, and accompanied with a clear ſky. My thermometers are ſix feet from the ground, in an arbour in the garden, ſhaded from the ſun, and almoſt from the wind. The arbour fronts the north, and is ſurrounded by high walls on one ſide, and houſes on the other, at about thirty yards diſtance. On Feb. 19th, at 7 A. M. the mercury ſtood at 25°.—3 P. M. at 50—12 P. M. at 30—

Feb. 20—7 A. M.—at 22½ —3 P. M.—at 49——12 P. M.—at 28
——21—7 A. M.—at 25——3 P. M.—at 48——12 P. M.—at 30
——22—7 A. M.—at 26——3 P. M.—at 53½ —12 P. M.—at 30
——23—7 A. M.—at 27——3 P. M.—at 54——12 P. M.—at 32
——24—7 A. M.—at 27——3 P. M.—at 57½ —12 P. M.—at 31
——25—7 A. M.—at 31——3 P. M.—at 54——12 P. M.—at 36½
——26—7 A. M.—at 34½ —3 P. M.—at 47——12 P. M.—at —
——27—7 A. M.—at 31½ —3 P. M.—at 44———— ——
——28—7 A. M.—at 22——3 P. M.—at 40½ —12 P. M.—at 32

The firſt week of March it did not deſcend below 32° except one morning, though it kept pretty near it. The middle of the day was warm, and without rain, as was the caſe till the equinox.

This little ſketch of our weather will account for the univerſality of catarrhal affections among us, accompanied with more inflammation than for the moſt part happens in common colds. In many caſes they were truly pneumonic, and proved ſuddenly fatal, eſpecially to elderly people. The fineneſs of the day cauſed both the morning ſpencer and the evening ſurtout to be neglected; and in eight hours, as you ſee, they were carried through a range of temperature ſometimes exceeding 35 degrees, beginning with 10 degrees of froſt, and in the ſucceeding eight hours, plunged back into a degree of cold not much leſs than that of the morning.

On the 27th of February, a little boy who has lived with me in the capacity of a ſervant for about a year paſt, and is my guide when I walk out, and reads and occaſionally writes for me when at home, was ſent between three and four P. M. with a newſpaper to the next ſtreet. He had been affected with this complaint for about ten days, as all the family were, and his cough with febrile heat was conſiderable, eſpecially the former. He is about ten years old; is delicate and even ſmall of his age, and apt from a playful diſpoſition and lively turn, to run the rig with the boys in the ſtreet, in place of going on my errand when I ſend him out, ſometimes not returning for two, three, or more hours. As he is uſeful to me, and an orphan left to the care of the pariſh, I indulge him conſiderably, beſides being his preceptor. When ſix ſtruck, John was not returned; eight came, no John appeared; nine, and no diſcovery made of him. The night, as you perceive, was very cold; his complaint ſomewhat alarming, and I under conſiderable uneaſineſs leſt it ſhould increaſe from his being heated, as I preſumed, by play, and ſuddenly cooled by the froſt. When eleven came, the meſſengers I ſent returned without finding him, but with the intelligence of his having been ſeen at play about twilight. It was in vain to ſearch farther at this late hour, and I concluded he muſt have gone with ſome playfellow for the night, for fear of chaſtiſement for his neglect. When morning appeared, as ſoon as perſons could be got, I ſent again in ſearch of him, but it was eleven o'clock before he was diſcovered, running out of town with a croud of boys after the ſoldiers who were about to be reviewed. On interrogating him at his return reſpecting his abſence, he confeſſed fear of puniſhment prevented his return, from a conſciouſneſs of miſconduct. He poſitively aſſerted he had been in no houſe ſince the time he left me till his return now; that he had walked the ſtreet the whole night, a little time excepted, when he ſat down on a ſtone ſtair leading to a ſhop, to reſt, but did not ſleep; that when day appeared he repaired to the barracks, and on the gates being opened, went in and walked about till parade hour. He followed the ſoldiers to roll-call, and thence to the field, on the way to which he was diſcovered. The mud on his great coat, and his diſhevelled hair, beſpoke he had not been in bed. The night, as you have ſeen, was one of the coldeſt we experienced ſince December, and even then exceeded by one or two nights only;* for at 7 A. M. during his perambulation, there was 10 degrees of froſt, and at five, which I preſume might be the coldeſt part of the morning, we may fairly allow the air to be cooled ſtill two degrees more. John's ſtory was artleſs and ſimple, and I could not doubt the truth of his relation. Inſtead of puniſhment, which I thought he had ſeverely felt already, he received my ſympathy. During the interrogation he underwent, and which took up a conſiderable time, I obſerved he did not once cough although in the ſame ſpace of time ſince the commencement of his catarrh, it would have often harraſſed and interrupted his narrative.

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* December 23, 1796, Mercury ſtood at 26°—At 8 A. M. next morning, at the same hour it came down to 16—At 8. P. M. of ſame day it ſtood at 15.—An hour after it came down ſo low as 10.—At 7 in the morning, Dec. 25, it had ſunk to 2. two feet from the ſurface of the earth. At 10 A. M. it had aſcended to 12—At 4 P. M. it was 21½ At 6 P. M. it ſtood at 23—At 8 P. M. it ſunk again to 21. Two hours after it roſe to 28. and next morning at 8 it was at 31.—conſequently, the night this boy walked the ſtreets was colder throughout than any night of this remarkably cold month, one only excepted.

Twenty hours had now elapſed, without food or drink, and expoſed to an atmoſphere of great cold, and yet he ſeemed nothing affected, except from the temporary fear of the rod.

I watched him narrowly, as I apprehended a fever might be the conſequence of his midnight ramble; but here I was agreeably diſappointed, for in place of fever his catarrh was cured, his cough ceaſed, and never after returned. He had ſome food given him, but it was ſparingly, and ſimple of its kind, being a little plain peas-ſoup, with ſome fiſh and potatoes; and I reſtrained him from the warmth of the fire, by ordering to an out-houſe to clean ſome knives, judging it unſafe for him to remain in a heated room in his preſent ſtate of accumulated irritability; and even when night came I was under apprehenſion from the effect of a warm bed. I had ſome years ago ſeen the bad effects of ſudden warmth ſucceeding cold; or in other words, warm cloathing exchanged for rags and nakedneſs. It was the caſe of Watſon, publiſhed in my Reg. Surg. Ed. 2d. 1794, a vagrant who had been a drummer in the regiment where I ſerved, and who found me out here in the courſe of his ramblings. I cloathed him completely. The waiſtcoat was compoſed of warm materials, lined with flannel, which I uſed to wear on horſeback through the winter. The day he put them on was a mixture of rain, ſnow, and ſleet, which had buffeted his bare body in ſeveral parts. Next day he was extremely ill, with catarrh and fever; and the ſucceeding day his complaints increaſing, he threw off the warm cloathes, and betook him again to his rags, ſwearing the warm cloathes had killed him.

With reſpect to the cure performed on my boy, I ſhall leave it to you to ſupply explanatory reflections. We cannot ſend our patients, when they apply to us under this complaint, to walk abroad in froſty weather; but we can forbid them their warm drinks, warm rooms, increaſed quantity of bed-cloathes, &c. &c.

R. H.

In ſome of our older medical authors, there may be found on the ſubject of temperature, very valuable collections of facts of which we have not yet fully availed ourſelves. The facts themſelves can hardly be rejected as fabulous. They are too numerous, diſtinct, independent of ſpeculation, and (as far as the compariſon can be made) conſonant with our lateſt and moſt accurate obſervations. Dr. Edward Baynard, who honeſtly owns them to "be paſt of his philoſophy," gives the following relation. It is the moſt intereſting of the kind I have met with, and may in many dangerous caſes (particularly of the natural ſmall pox) be uſeful to modern practitioners.

"In *fevers* I have known a great many in my time, who by the over-care of their *health-wrights* were made *delirious*, and in their phrenſy have leaped into a *pond*, or any other *cold water*, and not *one* as ever I heard of got any harm, but were thereby preſently cured. And Dr. *Willis*, I remember, inſtances a caſe or two, wherein they have recovered by immerging into *cold water*, either by accident or diſtraction. And lately I ſaw at Mr. *Charles Frubſhaw's* in *Saliſbury Court*, a ſervant maid, who not long before being delirious in a moſt intenſe fever, got looſe and leaped into the river *Thames*, but being ſoon taken up by a boat, was brought home in her wet cloathes, who no ſooner being ſtript and dry cloathes put on, but ſhe went about her buſineſs, and was as well as ever ſhe was in her life. I had often heard this ſtory in the neighbourhood, but being curious in the thing, I ſent for the maid, and had this relation from her own mouth.

" A learned and ingenious gentleman, a doctor of laws, now living told me, that being light-headed in a fever, and moſt intenſely hot and thirſty, got from his nurſe, and ruſhed into a horſe-pond in the yard, and there ſtayed above half an hour; it brought him preſently to his ſenſes, and allayed both his heat and thirſt. After which, when in bed, he fell into a ſound ſleep, and when he awaked (in a great ſweat) he found he was well, but complained of a great pain in his head for ſome time after, which he himſelf thinks proceeded from not wetting his head.

" Mr. Carr, the preſent ſchoolmaſter of Marlborough, told me, that he recovered when given over in a fever, by drinking a large quantity of cold ſpring water. And that I have known in twenty ſuch caſes in my time, but that is not to be depended upon, for ſome have alſo recovered by a quite contrary method, as drinking ſtrong fermented liquors, as cyder, ſack, claret, &c. in large quantities. See *Harmanus vander Heyden de uſu aq. fontanæ & ſeri Lactis.*

" A Turk (a ſervant to a gentleman) falling ſick of a fever, ſome one of the tribe of *treacle-conners* being called in, whether Apothecary or Phyſician, I cannot tell, but, (according to cuſtom) what between bliſter and bolus, they soon made him mad. A countryman of his, that came to viſit him, ſeeing him in that *broiling* condition, said nothing, but in the night-time by some confederate help got him down to the Thames-side, and ſoundly ducked him. The fellow came home ſenſible, and went to bed, and the next day he was perfectly well. This story was attested to me by two or three gentlemen of undoubted integrity and worth ; and I doubt it not, but believe it from the greater probability ; for I will hold ten to one on the Thames-side against treacle, snakeroot, &c. and that all hot regimen, which inflames and exalts the blood, breaks its globules, and deſtroys the man, and then forſooth the doctor ſneaks away like a dog that has loſt his tail, and cries it was a peſtilential malignant fever, that no body could cure, and ſo ſhews his care of the remainder, bids them open the windows, air the bed-cloaths, and perfume the room for fear of infection, &c. And if he be of the right whining, canting, prick eared ſtamp, concludes as they do at Tyburn, with a mournful ditty, a pſalm, or a preſervative prayer for the reſt of the family, &c. ſo exit Prig, with his ſtarched formal chops, ebony cane, fringed gloves, &c.

"Dr. Yarborough told me, that his kinſman, Sir Thomas Yarborough, ſent him a letter from Rome, wherein he gave him an account of a footman of his, who when delirious in the ſmall-pox, got from his bed, and in his ſhirt run into a grotto of a Cardinal's, where there was water, in which he plunged himſelf, but was preſently got out: the ſmall pox ſeemed to be ſunk and ſtruck in, but upon his going to bed they came out very kindly, and he ſafely recovered.

"But my learned and worthy friend Dr. Cole, ſhewed me an account from an Apothecary in Worceſterſhire, whoſe name (I think) was Mr. Matthews; the ſubſtance of which was, that a young man delirious in the ſmall pox, when his nurſe was aſleep, jumped out of bed, run down ſtairs, and went into a pond; the noiſe awaked the nurſe, who followed with an outcry, which outcry raiſed the poſſe of the family, who ſurrounded the pond; but he parled with them, and told them, that if any body came in he would certainly drown them, and that he would come out when he ſaw his own time; and accordingly did ſo, and walked up ſtairs, and ſat (in his wet ſhirt) upon a cheſt by the bed-ſide; in which poſture Mr. Matthews found him when he came into the chamber. Note here, that the Apothecary lived three or four miles from the place, and he was in the water and on the cheſt all that while in his wet ſhirt, that the meſſenger was gone for him. This apothecary, Mr. Matthews (for ſo I take his name) asked him, how he did? He anſwered, pretty well. He aſked him, if he would have a clean ſhirt, and go into bed? He ſaid, by and by he would; which accordingly he did. When in bed, he aſked the apothecary, if he had nothing good in his pocket, for he was a little faintiſh? He ſaid that he had a cordial, of which he drank a good draught, ſo went to ſleep, and awaked very well, and in a little time recovered. Now, as Dr. Cole obſerved very well, a man, quoth he, would not adviſe his patients in ſuch a caſe to go into cold water, though this man eſcaped without injury; but it gives a good occaſion to reflect on the many miſchiefs that attend the ſmall pox in the hot regimen, ſince ſuch extravagant, and intenſe cold does ſo little or no harm.

" Dr. Dover of Briſtol, told me of a Vintner's drawer in Oxford, that in the ſmall pox went into a great tub of water, and there ſat, at leaſt two hours, and yet the fellow recovered, and did well.

" A gentleman delirous in the ſmall pox, run in his ſhirt in the ſnow, at leaſt a mile, and knocked them up in the houſe where he went, they being all in bed ; the ſmall pox ſunk, yet by the benefit of a looſeneſs he recovered.

" I remember about two years ſince, a learned gentleman, a divine, told me, that in the country where he was beneficed, in a ſmall town, not far from him, many died of a malignant ſmall pox. A certain boy, a farmer's ſon, was ſeized with a pain in his head and back, vomited, was feveriſh, &c. and had all the ſymptoms of the ſmall pox. This youth had promiſed ſome of his comrades to go a ſwimming with them that day, which notwithſtanding his illneſs, he was reſolved to go, and did ſo, but never heard more of his ſmall pox. Within three or four days, the father was ſeized juſt as the ſon was, and he was reſolved to take Jack's remedy ; his wife diſſuaded him from it, but he was reſolved upon it, and did immerge in cold water, and was after it very well. The worthy gentleman that told me this ſtory, promiſed to give me it in writing, with the perſons names and place; but I neglecting of it, he went out of town in two or three days, ſo I loſt the opportunity of being better informed.

"Mr. Lambert, brother to my worthy friend, Mr. Edmond Lambert of Boyton, in the county of Wilts, told me, that when he was at ſchool in Dorſetſhire, at leaſt thirty or more of the boys, one after another, fell ſick of the ſmall pox, and that the nurſe gave them nothing elſe but milk and apples in the whole courſe, and they all recovered. There was but one diſſenting boy from that method, who by command from his parents, went another courſe, and he had like to have died; nay, with very great difficulty they ſaved his life. And ſince, another gentleman told me, that himſelf and divers others were cured by milk and apples, and buttered apples, in the worſt ſort of ſmall pox.

"I was at Chiſwick, and ſometimes in London, in the time of the great plague, in the year 1665; and I very well remember, that it was the talk of the town, that a brewer's ſervant at Horſleydown in Southwark, was ſeized with it, and in his delirium run into a horſe-pond, firſt drank his fill, and then fell faſt aſleep with his head upon the pond's brink, where he was found in the morning; how long he had been in the pond, no body knew, for it was in the night he went into the water, and had no nurſe then with him, but he recovered to a miracle.

"I heard alſo about that time of a nurſe taken with the plague, that accidentally fell into a well, ſomewhere near Acton, and was immediately brought to her ſenſes and recovered. I was told this by ſome Acton men."

Floyer and Baynard's pſychrolugia, p. 226—232.

In reſpect to the cold bath in the yellow fever, though, as Mr. Yonge ſays, it did not come early into queſtion at Philadelphia, the phyſicians at New York employed it much, as we find in various works, as Hoſack's Eſſay, New York, 1797, and the medical repoſitory, No. 2, p. 233; where it is ſaid that cold water, thrown on the patient in the firſt days of fever, reduced the pulſe in frequency and force, diffuſed a general and pleaſant coolneſs, and mitigated or removed pain. Theſe changes laſted in proportion to the extent to which the cold affuſion was carried, and to ſome other circumſtances. Afterwards the heat, pain, and fever returned, ſo as to become worſe than at firſt. Towards the cloſe of the diſeaſe, cold affuſions are ſaid never to have occaſioned more than a ſlight and temporary return of warmth. "More "often the ſkin has continued cold; and we "have been certain that ſuch a torpor of the "ſyſtem has been induced, as has aided the "progreſs of the diſeaſe. On the contrary, "when (in the firſt days of the fever) while the "heat was great, the pulſe ſtrong, and the "ſtrength of the body not yet reduced, we "have noticed, that a gradual application of "cool, or even cold water, ſo that the whole

" body ſhould be waſhed, has had very happy " effects, eſpecially *if perſevered in and fre-* " *quently repeated.* Like beneficial conſe- " quences reſult from ſimilar applications, " ſimilarly conducted, towards the cloſe of the " diſeaſe, if the morbid heat of the ſkin con- " tinue. Nor is it of any great importance " whether the water be cold or tepid, if the " patient be freely expoſed to the air. . . Thus " the ſtrong pulſe of the firſt ſtage is diminiſhed " in force; the weak pulſe of the laſt in fre- " quency, and perſpiration or ſweating follows. " That this is the fact, whether the fever be " inflammatory or putrid (to uſe the ordinary " terms of medical writers) is confirmed by the " experience of thoſe who have written from a " careful attention to facts happening under " their own eyes."

In the ſcarlatina, I have known excellent effects from free expoſure of the body to the cool atmoſpheric air, and in this diſeaſe have recommended it to lay children upon ſingle linen, ſtretched and ſupported by treſſels. In the croup I have kept children with deciſive advantage in cold air. Here alſo I doubt not but conſtant cold applications to the throat would do ſervice.

But what would ſave multitudes (particularly of children, who periſh becauſe neither parents nor medical men, ſuch as are generally firſt called to them, have any proper conception of the effect of temperature on their diſorders) is an apparatus for applying heat or cold at pleaſure to the whole body or any part of it.

To perſons ſubject to cold feet, I have long been accuſtomed to recommend, in the day time, a tin veſſel capable of holding their feet, and having double ſides, the interſtices between which are filled with hot water. I am informed that *footwarmers* of a more convenient conſtruction than mine are now made by Lloyd, near Norfolk-ſtreet, Strand, London.

MISCELLANEOUS

REPORTS AND OBSERVATIONS

Concerning the reſpiration of

GASES AND VAPOURS.

Since the publication of the laſt part of CONSIDERATIONS ON THE MEDICINAL POWERS OF FACTITIOUS AIRS, *various reports, more or leſs favourable, have been tranſmitted to the editor; and he has been offered communications which would have filled two or three volumes. He however thought it right to decline theſe obliging offers, and to forbear publiſhing any thing, till the eſtabliſhment for the inveſtigation of the powers of theſe ſubſtances ſhould preciſely aſcertain facts. In the* CONSIDERATIONS, *enough was ſtated to exemplify the practicability of the occaſional uſe of ſmall quantities of modified air: and ſome of the reports convincingly prove the efficacy of the treatment. . See in particular the caſe of Mr. Atwood, p. 57, Part i. Edition 3d. and other ſimilar cures of ſore legs. But in moſt inſtances, further and more elaborate reſearches are undoubtedly wanting to determine what ſhare the gaſes had in the favourable reſult—and the editor's view in publiſhing them was to encourage the trial of theſe means where all others are avowedly unavailing.*

The three following communications are ſelected from among many others; and the ſubjoined ſentiments of foreign phyſicians cannot but be acceptable to the curious Engliſh reader.

Letter from MR. CREASER, *Surgeon, Bath.*

Dear Sir,

The diſeaſe which I am about to communicate to you the treatment and event of, appears to me to have been ſo anomalous as not to have been exactly deſcribed by any noſologiſt, though it reſembles moſt the ſtomacace of Sauvages, Order 5, genus 27, ſpecies 2. The ſubject of it was a young married lady who had been generally chlorotic, and in whom the diſcharge, when it occurred, was remarkably ſmall and ſerous. After marriage ſhe became pregnant ſeveral times, but always miſcarried, and a perceptible degree of chloroſis was conſtantly manifeſt with its uſual ſymptoms.

Some years ago a ſlight herpes appeared on the face, in the vicinity of which effuſions of blood took place into the cellular membrane, with enlargement of the capillary veſſels. Thoſe effuſed parts generally diſcharged themſelves by bleeding externally, and ſo frequently that I have known thirty ounces of blood loſt by this way in a week, and ſeldom leſs than ſix or ſeven ounces. I believe that the hœmorrhages which occur as a relief to the other diſeaſed actions, very frequently exceed the bounds of what is neceſſary, and become in themſelves diſeaſes. This is often the caſe in hœmorrhoids and bleedings from the noſe.

I conſidered this diseaſe as dependent on the deficient menſtrual diſcharge, and adminiſtered a variety of the uterine ſtimulants, as they are called, without effect. Steel conſtantly improved the general vigor of the ſyſtem, and as uniformly in proportion increaſed the hœmorrhage from the capillaries of the face. The uſual ſyſtems of treatment by mercury and other remedies, as well as the moſt aſtringent and ſtimulant topical applications, had alſo been ineffieaciouſly employed.

Under these circumſtances, and when the diſcharges were both frequent and conſiderable, I began to adminiſter oxygen gas in the accuſtomed way. Soon after its adminiſtration, the ſymptoms dependent on the chlorotic ſtate of the ſyſtem were conſiderably amended. The difficulty of breathing, and oedema of the legs, which had ſubſiſted in a conſiderable degree, were nearly removed, and more perfect ſleep was induced. After its uſe for a month, by which time three quarts of oxygen were given three times a day, a more healthy colour of the ſkin ſucceeded, and the livid colour and vaſcular appearance of the effuſed parts began to diſappear. The hæmorrhage was ſo much diminiſhed, that it was thought neceſſary to inſert an iſſue for the purpoſe of obviating the effects of the ceſſation of an accuſtomed diſcharge of ſuch magnitude.—This was the only additional remedy employed; and leſt any improper ſtreſs may be laid on its auxiliary effect, I muſt obſerve that it was not adopted till after the amendment was conſiderable, and that it had before been employed ſingly without any effect.

The oxygen was continued in the ſame

quantity for three months, with progreſſive amendment, but not with entire ceſſation of the diſcharge. It was then omitted, from a ſuppoſition that it might loſe its power from habit ; but after the intermiſſion of its uſe the improved health of the ſyſtem continued, and the diſeaſe gradually diſappeared.

The ſingular concluſions which appear to me to be deducible from this caſe are theſe.—The oxygen increaſed the fulneſs, frequency, and ſtrength of arterial action, and the heat of the body, but did not increaſe the diſcharge, as ſteel, exerciſe, and a full living did. We muſt either, therefore, explain the effects of oxygen by the opinion of the late Mr. Hunter, that action and ſtrength are eſſentially different, and that it is one of the few ſtimuli which confer both ; or in the opinion of ſome more recent phyſiologiſts, we may ſuppoſe that oxygen communicates irritability to the ſyſtem, and thus affords a ſource of excitement. This appears to be confirmed by oxygen's rendering the body more ſuſceptible of other ſtimuli, as purgatives, of which ſome facts are in my poſſeſſion. I remain, dear ſir,

Your obliged and reſpectful ſervant,

THOMAS CREASER.

Dr. BEDDOES.

Letter from Dr. CROWTHER.

WAKEFIELD, 1798.

To Dr. BEDDOES,

Sir,

It gives me great pleaſure to be able to add one to the many favourable teſtimonies which you have publiſhed, reſpecting the efficacy of pneumatic medicine. Nothing can be more deciſive than the curative effect of hydro-carbonate gas, in the ſubſequent caſe of Mr. Orange.

Much has been ſaid by the enemies to innovation in medicine, reſpecting the inaccuracy and miſtatement of ſome of the caſes which have been ſent to you. It has been inſinuated, that ſome of them have been fabricated by young practitioners with a view of bringing themſelves into notice. That no ſuch implication may be attached to the preſent caſe, I hereby pledge myſelf to introduce to the patient, any neighbouring medical practitioner into whoſe hands the caſe may fall, who being a ſceptic reſpecting the efficacy of factitious airs, may wiſh for further teſtimony on the ſubject.

I was called to ſee Mr. Orange, after a veteran in phyſic had declared the caſe to be conſumptive and incurable. When I was interrogated by his friends reſpecting the prognoſis, I anſwered, that according to the uſual mode of treatment, the proſpect of recovery was very unfavourable; but that ſome cures had lately been performed in ſimilar caſes, by the uſe of factitious airs. The ſucceſs far ſurpaſſed my moſt ſanguine expectations.

Caſe of Pulmonary Abſceſs.

Oct. 28th 1797, Mr. Joſeph Orange, grocer, Wakefield, a married man, aged 26; is affected almoſt inceſſantly with cough and copious expectoration. Laſt night he began to ſpit up large quantities of thick, fœtid, browniſh, purulent matter, which ſunk in water, and rendered the room in which he ſat very offenſive: his pulſe is about a hundred in a minute, and of moderate ſtrength; he is affected with hectic heats in the afternoon, and ſweating in the morning. Bowels rather looſe, countenance pallid, appetite and ſtrength much impaired, tongue foul. He is much emaciated, his eyes appear hollow, and his cheek bones very prominent. He has been confined to his room about a fortnight, but has expectorated mucus ſtreaked with blood, and been affected with cough for ſome time paſt. He has been ſubject to cough, pain in his ſides, and dyſpnœa at times, for ſeveral years.

He was unuſually dejected and oppreſſed laſt night about the precordia, before the purulent expectoration commenced. I ordered a mucilaginous mixture for the cough, and a bolus to be taken at bed time, with a grain of opium, a grain of balſam of tolu, and five grains of alum; and directed him to inhale twice a day, a pint of hydro-carbonate gas, mixed with twenty-four quarts of atmoſpheric air.

29th. Slept better laſt night than he has done for ſome time paſt; felt no vertigo from the hydro-carbonate gas, but thought himſelf refreſhed by it. Cough leſs frequent. Ordered him to continue his former medicines, to take five grains of ipecacuanha in the morning, and to live principally upon good milk.

30th Was again much dejected and oppreſſed about the prœcordia laſt night, which as before, was followed by a very copious expectoration of purulent matter during the night. He ſlept ill on account of the cough; the emetic operated gently. To continue his medicines.

31ſt. Had a tolerable night, but ſweated profuſely this morning; cough leſs troubleſome; expectoration ſomewhat diminiſhed. Says that he always finds himſelf refreſhed after breathing the reduced atmoſphere, and feels diſappointed and uneaſy when it is not adminiſtered to him at the uſual hours. I ordered him to increaſe the hydro-carbonate to a pint and half for a doſe, to riſe out of bed when the ſweats come on, and take twenty drops of elixir of vitriol in water. I directed him to have an iſſue in his ſide capable of containing four peas, and to uſe exerciſe frequently in a ſwing erected in his ſitting room for that purpoſe.

5th Nov. Cough, expectoration, and ſweating, are much diminiſhed; he uniformly finds himſelf refreſhed after breathing the hydro-carbonate air, which has been increaſed to a quart for each doſe. Pulſe 94. I have the ſatisfaction to find that my directions have been implicitly complied with. Ordered him to continue his former medicines, and to take a powder twice a day, containing gum myrrh eight grains, vitriolated iron one grain and half, powder of colombo root ten grains.

10th. He continues gradually to recover; appetite much improved; dyſpnœa and ſweatings nearly gone; cough much leſs troubleſome; expectoration much diminiſhed. Ordered him to continue his medicines, and to take two pints and half of hydro-carbonate for a doſe.

17th. He has recovered very rapidly ſince laſt report; his cough and expectoration are nearly gone; he gains fleſh and ſtrength daily; his appetite is greater than uſual when in health. After running up two pair of ſtairs, his pulſe beat only eighty in a minute. He breathes more eaſily than he has done for ſeveral years before.

Dec. 25th. Mr. Orange is now in perfect health.

Jan. 3d 1798. By acting in every reſpect contrary to the directions which I had given him, from the idea that he was ſo well that nothing could poſſibly hurt him, Mr. Orange has brought on a return of his cough with copious expectoration. He has expoſed himſelf without proper covering to the inclemency of the weather, and has enjoyed at late hours, and in crowded rooms, the convivial meetings uſual at this ſeaſon of the year. By a recurrence, however, to his former mode of treatment, in the courſe of about a fortnight, his health was completely re-eſtabliſhed, and he now remains perfectly well.

Feb. 14th 1798.

Caſe of Phthiſis Pulmonalis, or as ſome authors would denominate it, Atrophia Lactantium.

May 2d 1797.

Catharine Grey, a diſpenſary patient, aged about 36 years; was attacked three weeks ago with flying pains in the cheſt, and cough, attended with copious expectoration of frothy mucus; her tongue was foul, ſhe had ſome thirſt, her pulſe was 110 in a minute; her appetite was much impaired, and ſhe had regular hectical exacerbations in the afternoon. About two months ago ſhe was delivered of twins, which ſhe has ſince nurſed, though ſhe takes but little ſood, and that of a very coarſe kind, being extremely poor.

She has had a bliſter applied to the ſternum, which occaſioned a profuſe diſcharge and great debility, with very little relief to the ſymptoms. I directed her to live principally upon a milk diet, to diſſolve one dram of lozenges, containing one grain of opium, in her mouth at divided intervals daily: to take a ſcruple of peruvian bark, mixed with a ſcruple of colombo root, thrice a day, and to inhale one dram of the vapor of ether, in an ounce of which half a dram of the dried leaves of cicuta had been infuſed thrice a day.

12th. She appears to have loſt ſtrength conſiderably ſince laſt report; her cough is very troubleſome, and ſhe expectorates large quantities of mucus ſtreaked with blood. In a morning the matter expectorated is heavy, at other times light and frothy. The bark diſagrees with her ſtomach, but ſhe imagines that ſhe finds conſiderable relief from the vapor of the etherial tincture of cicuta. I was not acquainted until this day, that ſhe ſuckled her twin children. She is ordered to wean them immediately, to omit the bark, and take inſtead thereof, one ounce of a mixture compoſed of decoction of bark and bitter infuſion, every three hours, and an opiate at bed-time. The vapor of ether to be continued. She will now be regularly ſupplied with as much good milk as ſhe can uſe.

23d. She imagines that ſhe finds relief from the ether, but is evidently weaker ſince laſt report. She has been attacked with diarrhœa and colliquative ſweats., and expectorates as much as formerly. She is ordered to omit the decoction of bark, to continue the ether and opiate, and to take one ounce of the cretaceous mixture along with one dram of the tincture of catechu after every looſe ſtool.

30th. The diarrhœa has conſiderably abated; ſhe feels a good deal of pain in her breaſt from coughing, in other reſpects ſhe is much the ſame; the appetite is weak. She is ordered to continue the ether and opiate, to have an iſſue in her ſide, and to take the following powder thrice a day; colombo root one ſcruple, gum kino and myrrh each eight grains.

June 14th. Her cough is leſs frequent, her expectoration is diminiſhed in quantity, and ſhe thinks herſelf ſtronger: her pulſe is ſtill quick, and ſhe continues to perſpire much in the night. Ordered to repeat her former medicines, and to take forty drops of diluted vitriolic acid every night at bed-time.

21ſt. She has recovered rapidly ſince laſt report, the colliquative ſweats have ceaſed; her cough, expectoration, and hectic heats diminiſh daily. Pulſe about 90, appetite and ſtrength much improved, ſhe takes milk very freely. Ordered to continue her medicines.

28. She ſtill recovers rapidly. Ordered to continue the ether, and to take the following powder thrice a day: colombo root half a dram, myrrh eight grains, vitriolated iron one grain mix.

July 2d. On the 30th of June, the catamenia appeared in unuſual quantity, and weakened her much; ſhe diſcharged likewiſe by coughing, a piece of grumous blood about the ſize of a hazel nut. Ordered to omit her medicines, and continue the etherial tincture only.

July 10th.—She continues to recover her fleſh and ſtrength very faſt, her hectic heats and ſweats have left her; her pulſe is about 80 in a minute. Her cough is nearly gone. She attributes her recovery entirely to the etherial tincture of cicuta. By the end of July ſhe was perfectly well, and from a state of extreme macilency became obese. She ſtill continues very well Dec. 10, 1797, ſince which period I have not ſeen or heard of her. The diſeaſe was undoubtedly brought on by want of the common neceſſaries of life; ſhe had neither ſufficient food nor clothing: the matter of doubt with many will be, whether her recovery ought to be attributed to the nutritive diet which ſhe obtained after I ſaw her, or to the vapor of ether and cicuta. I am clearly of opinion that her recovery could not have been effected by either one or the other ſeparately, but is to be attributed to their conjoint effect. I deem it neceſſary, however, to add, that I have uſed ether in three other caſes not apparently more unfavourable, without permanent advantage. I have uſed hydrocarbonate air along with diuretics in a caſe of hydrothorax, complicated with a tuberculous affection of the lungs, with much advantage. I at firſt gave vital air which aggravated the ſymptoms. Make what use you pleaſe of the contents of this letter.

I am,

Your humble Servant,

CALEB CROWTHER, M. D.

Memorandum from a Friend to Miss NORTON.

As it is only from recollection that an account of Martha Norton's caſe can be given, I fear it will not be ſo minute and accurate as Dr. Beddoes may wiſh.—During laſt ſummer (1796) ſhe was frequently indiſpoſed and, every little exertion, even walking up ſtairs, brought on an oppreſſion of breath and great laſſitude.

A journey to London in Sept. was followed by great fatigue and difficulty of breathing, from which ſhe did not recover for ſome weeks. Early in the winter ſhe had a bad cold and cough, and in January 1797, a cold renewed the cough, which ſoon became violent and almoſt inceſſant, attended by ſo great oppreſſion of breath that ſhe could not lie down, and intire loſs of appetite; the expectoration was great, and ſunk in water. She was bled and a bliſter put on her cheſt without affording any relief.—Nine days after the appearance of theſe ſymptoms, the Apothecary who attended her acknowledged to her friends, that he thought her caſe did not admit of hope; but recommended for their ſatisfaction, that a Phyſician ſhould ſee her; Dr. Saunders confirmed the Apothecary's opinion, and ſaid nothing could be done but to alleviate the moſt diſtreſſing ſymptoms. A milk diet and a change of air were preſcribed as ſoon as ſhe could bear a journey, and a medicine given which relieved the ſpaſms (they had been ſo violent as to make the Apothecary apprehenſive of convulſions in which he thought it probable ſhe might ſuddenly expire) the hectic continued unabated, the night ſweats profuſe, and the expectoration purulent.

This was her ſtate when the air was firſt adminiſtered. She took twice a day, a quart of the hydrogene diluted with fifteen quarts of common air. For the two or three firſt days it occaſioned a ſlight degree of dizzineſs, and depreſſion of ſpirits. The pulſe was immediately ſofter and leſs frequent. The expectoration leſs and likewiſe better in quality, the cough gradually leſſened; and ten days after uſing the air ſhe was intirely free from it, and from every other alarming ſymptom. Her ſpirits and appetite returned, and though during the winter and ſpring ſhe has had frequent colds from the variableneſs of the weather, they have affected her much leſs than might have been expected, which is attributed to the conſtant uſe of the air, till a fortnight ſince.

A cold taken in the interval cauſed a return of cough and hectic, from which ſhe is again relieved by returning to the uſe of the air.

The Apothecary who attended Martha in London, has been deſired to give a more particular ſtate of the caſe, which as ſoon as received ſhall be ſent to Mr. K. It is as follows:

Miſs M. NORTON of a dark complexion, and a frame ſo delicate, that mechanical aid was neceſſary to prevent a diſtortion of the ſpine, yet the pupils of the eyes were quick and intelligent without any remarkable dilatation; had been afflicted with ſtrumous inflammation and diſcharges from the ſubmaxillary glands in a more early period of life. She is at this time in her eighteenth or nineteenth year; and was free from any conſtitutional irregularity or ſuppreſſion whatever.

About the 20th of January, 1797, was attacked with ſlight fever and troubleſome cough; on the 28th I firſt called on her, and found her complaining of great oppreſſion and ſlight pain in her breaſt, a cough dry and troubleſome, paticularly on lying down, with great debility, ſlight though not conſtant head ache, conſiderable heat of ſkin, want of appetite, and a pulſe at 110. An antimonial medicine was given every ſix hours ſo as to act as an emetic, and conſiderably increaſe the diſcharge on the ſkin. On the 29th, a bliſter was applied to the ſternum, and pectorals with ſalines and an opiate were given; but without any ſenſible benefit. On the 30th, about five or ſix ounces of blood were taken from the arm with no better ſucceſs, nor could I obſerve that the ſtrength was impaired by it. Next day but one, Feb. 2, ſhe was viſited by Dr. Saunders, who adviſed her the myrrh draughts in pectoral emulſion every ſix hours and ſtorax pill at night. Theſe medicines were continued with little variation or apparent advantage about ten days, the uſual mild plan of diet being obſerved. The expectoration during the whole of this time was in ſmall quantity and in no degree, that I could obſerve, purulent. The ſtate of the bowels was ſteady, nor was thirſt much complained of. At this time, about a fortnight from my firſt viſit, and three weeks from the firſt attack, an unexpected and marked remiſſion of fever took place, attended with a conſiderable and purulent expectoration which ſeemed, as far as I could judge, to come from a pretty extenſive ſurface, rather than from tubercles or detached ſpots, or ulcers. This diſcharge very ſoon leſſened and improved in appearance; in this ſtate of things it was when the hydrogen air was firſt adminiſtered; the fever, quickneſs of pulſe and heat having completely remitted for thirty ſix hours at leaſt. So that I am really unable to ſay in this caſe, whether the air had any ſhare in arreſting or reſiſting the progreſs of pulmonary conſumption with its fever,—a diſeaſe, the moſt miſerable, painful and fatal which attacks the human body, in the part of the world we live in.

The effects of the hydrogen air on Miſs Norton, were at firſt great debility with ſlight pain in the breaſt, and great giddineſs of the head, though this laſt ſymptom is not, I believe, uncommon, to weakly people, in forcibly drawing in common air through any kind of tube. I think myſelf warranted, however, in ſaying that Miſs Norton recovered more rapidly than any one I had ever ſeen, in a ſimilar ſituation, where medicated airs had not been adminiſtered. It is further neceſſary to obſerve, that Dr. Saunders, as well as myſelf, thought Miſs Norton's caſe had every appearance of confirmed pulmonary conſumption with very little hope of recovery; yet to ſome minds where the progreſs is ſo rapid, it always leaves hope that the diſeaſe is a ſlow peripneumony, and in this caſe the mind was influenced and alarmed by the ſtrumous appearances on each ſide of the neck.

ARTHUR ROBINSON.

37th Southampton Buildings,
May 6th, 1797.

The firſt perſon on the continent who in conſequence of what I had written, publiſhed any experiment with factitious air in conſumption of the lungs, was Dr. Girtanner. His patient expectorated matter, had a violent cough with compleat hectic fever and "colliquative bleedings from the noſe, which were with difficulty ſtopped." The colliquative diarrhœa had come on with violence, and his laſt phyſician had declared that he could not hold out above three weeks. April 3d, 1795, he was made to reſpire one quart of carbonic acid air with two of atmoſpheric out of a bladder provided with a mouth-piece that covered the mouth and noſe, and had two valves opening in oppoſite directions. The patient ſaid he felt as if a great load was rolled off his breaſt—he could now breathe more freely, which he was not able to do for a year before. On the right ſide (which was the painful and diſeaſed part) he felt a gentle warmth and an agreeable titillation. In a quarter of an hour his doſe of air was repeated a ſecond, and in half an hour, a third time. Each doſe procured agreeable ſenſations;

the free reſpiration continued. Pulſe ſmall, ſpaſmodic *(kramphaft,)* 124. Dr. G. willing as he ſays to follow my plan exactly, made his patient almoſt entirely abſtain from vegetables and live on ſmoaked fiſh and ſalted meat. (Here I muſt obſerve, that I never propoſed either ſuch a diet or the uſe of factitious airs but as expedients, worthy of trial in ſo hopeleſs a diſeaſe; by no means as remedies for whoſe efficacy I could vouch). Next day the patient thanked Dr. G. in the moſt affecting manner for the good night he had enjoyed. He had ſlept three hours quietly and had ſweated but little. The diarrhœa ſeemed on the decline, and there had been no bleeding. The reſpiration of mixed air being repeated as before, produced the ſame agreeable effects. This plan of diet was perſevered in till the ſixth of June, and three or four quarts of diluted carbonic acid were repeated twice every day, no medicine whatever being uſed. The amendment was progreſſive except during part of May, when the patient ſuffered from uneaſineſs of mind. This being removed, the caſe went on well. By the 5th of June the patient was got ſo well that Dr. G. propoſed to omit the air. From June 6th to July 8th, his ſtate was variable. His ſtrength however ſo much improved that in fine weather he could walk two, three or four hours—ſleep good, no night ſweats, ſome dry cough chiefly by day. Pulſe 90 in the morning—100, 120, in the reſt of the day—no fluſhing—no burning in the palms of the hands.—July 3, he quitted his diet—4th, he had chillineſs and coughed all night—9th, he complained of the left ſide; but the pain was removed by two or three days reſpiration of the mixed air. An opiate being added, by the middle of Auguſt the pulſe was natural, and all the ſymptoms but a very trifling cough gone. On the 27th of Auguſt the patient was ſtout enough to walk twenty (German) miles. At the end of his journey Dr. G. was informed of his being entirely well.

Dr. Hufeland, a writer of much authority in his own country, immediately afterwards publiſhed a paper ſtrongly recommending a proſecution of the trials with factitious air, and mentioning two or three caſes where carbonic acid air directly from an effervefcing mixture and vapours from ſoil carried into the patient's apartment, had procured ſome mitigation in the laſt ſtage of conſumption ; and one caſe in which following the plough had compleatly and permanently cured confirmed conſumption. Seltzer water with milk being the only other means uſed.

The progreſs of recovery in Dr. Girtanner's patient having been witneſſed by many of the phyſicians at Gottingen, produced a lively ſenſation. Further trials were made in Mr. Richter's hoſpital, and an account at large of theſe experiments was publiſhed in Dr. Muehry's theſis *on the uſe of the inſpiration of fixed air in conſumption* Goettingen, 1796. The method of Girtanner, the diet excepted, was tried in five caſes of conſumption and one of aſthma. In the former no recovery was effected : but ſome mitigation was for the moſt part experienced for a ſhort time after the inhalation ; ſometimes however the contrary effect took place. Little change was obſerved in the pulſe or the heat. In general no cough followed the operation, except during the exacerbation of fever. In one caſe the fetid ſmell of the expectoration was amended. In two caſes the night ſweats ceaſed for a time, but recurred. The aſthmatic patient was little relieved. The ſame Dr. Muehry afterwards publiſhed another caſe, in which, beginning with a quart of carbonic acid to two of common air eight times a day, he increaſed the doſe to three quarts of unmixed carbonic acid air every hour. No ſenſible effect was produced upon the diſeaſe, ſometimes the reſpiration of the factitious air was attended with difficulty. Dr. Hempel of Gottingen, relates a caſe in which the carbonic acid air was uſed for a few days in the very laſt ſtage of conſumption, with no other effect but unpleaſant ſenſations immediately after the firſt reſpirations; viz. faintneſs, anxiety, dyſpnoea, quickneſs and inequality of the pulſe. Theſe feelings ſoon ſubſiding, a burning was felt in the left ſide, which was the ſuppoſed ſeat of the diſeaſe. By degrees, the lungs became accuſtomed to the air, and no effect followed its uſe. The proportion of carbonic was then increaſed from one third to half; and the ſame uneaſy ſenſations were produced. Mr. Ehrhart, the botaniſt, towards the cloſe of conſumption, having taken carbonic acid gas much diluted, for ſeven days, left it off becauſe it ſeemed to diſagree. Dr. Buckner tried carbonic acid gas for four days in a caſe of phthiſis extremely far gone, and reports that it occaſioned pain in the cheſt with dyſpnoea.

It has been ſince ſtated, that Dr. Girtanner's firſt patient after his journey of twenty german miles on foot, had a violent relapſe; and that he died on the 20th of Nov. 1795. "The "ſtrongeſt proof, ſays the account, of the great "relief he experienced, was the firm faith he "had in the air to the hour of his death. He "beſpoke an apparatus and was about to dreſs "to go out to haſten the workman, when he "ſunk lifeleſs into the arms of his attendant. "It ſhould be told that he had in his childhood "an ulcer of his lungs, in conſequence of a "blow, which was apparently healed by the "*ſuc d' herbes*; and that almoſt every ſpring

“ he ſuffered from cough, dyſpnœa, and puru-“ lent expectoration, ſymptoms which commonly “ diſappeared on the approach of ſummer.” Dr. Girtanner mentions further, as a fact to the preſent purpoſe, the caſe a perſon of forty, exceſſively thin, who had been plagued with an obſtinate dry cough for four months, which would yield to no medicine, not even to opium. “ This man,” ſays he, “ aſſiſted me in my experiments with fixed air; and in its preparation was obliged daily to inhale a portion. In a fortnight his cough was quite gone and returned no more.”

Thus ſcanty is our foreign information. It ſeems ſurpriſing that a very obvious ſuggeſtion was never carried into practice. For as in a large proportion of the caſes the carbonic acid, taken in ſmall quantities and at long intervals gave relief, what was more natural than to try a continued application of the ſame power? Could more benefit be expected from the ſmall doſe of a quart, than what Dr. Muehry tells us was repeatedly experienced? viz. that patients within a fortnight of their death ſhould expreſs in the ſtrongeſt terms, their ſenſe of the alleviation they experienced. Dr. Kortum, the author of the work (reputed claſſical) *de vitio ſcrophuloſo,* and a buſy practitioner, by no means ſanguine in his expectations from new projects, has given ſome information, in my judgment, of greater value than the preceding direct trials of factitious gas; and has ſubjoined reflections that have my entire aſſent. "The following facts," ſays Dr. Kortum, tend in my opinion to confirm the uſe of mephitic gaſes in conſumption. The mineral waters at Aachen are known to contain ſulphurated hydrogen gas in larger quantity, and to depoſit more ſulphur than any water, hitherto deſcribed, in Europe. They alſo contain much carbonic acid. Both gaſes are continually exhaling from the numerous hot-wells and ſtreams, ſo that the hepatic ſmell extends over the greateſt part of the place, and is particularly ſtrong near the wells. It is an old obſervation, that ſulphureous vapours are wholeſome in conſumption;* and it is truly ſtriking to obſerve how few conſumptive patients are to be found in Aachen, while in the circumjacent country the complaint is very common. According to the moſt accurate information I have been able to obtain, conſumption is here very uncommon ; and I have known conſumptive perſons much relieved by ſettling in Aachen. Does not this depend on the gaſes from the hot ſprings, particularly the hepatic ? Should my idea be confirmed, a moſt convenient apparatus might be eſtabliſhed here for the reſpiration of this gas by phthiſical patients. Taking a few inſpirations of a mephitic gas at ſtated hours can do nothing. The gas muſt be mixed with atmoſpheric air in ſuch proportion that the patient can conſtantly remain in it. At Monjoye, ſix german miles from Aachen, a place that lies very high in the mountains, and where of courſe the air is very thin and piercing, conſumption is ſo frequent, that according to the recent obſervations of a reſident phyſician, *one half of the inhabitants are cut off by it.* Inflammatory pectoral complaints are much more common than in the neigbouring low country. This confirms Beddoes's obſervation concerning the greater frequency of conſumption, and of inflammatory diſorders in elevated ſituations."

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* The contrary opinion, I think, is more general, at leaſt in this country. Editor.

Whatever may be the dearth of adequate experiments, the propoſal for trying gaſes and vapours has created abundance of diſcuſſion abroad. It has obtained repeated notice from the contributors to the moſt popular journals in Germany; viz. the *Gottingen Review*, *Hufeland's Journal, the Salzburg Medical Review, and the Gotha Journal for inventions, theories, and contradictions in phyſics and medicine*; from which publications I have borrowed moſt of the preceding intelligence. The moſt reſpectable and celebrated of the medical philoſophers on the continent, have expreſſed ſentiments, favourable to the general deſign. Writers, otherwiſe liberal and candid, have inadvertently aſſumed that I propoſed the inhalation of gaſes as a remedy, confirmed by my own experience, whereas, I only urged their full trial. Nor from the publiſhed facts did I ever pretend to draw any but this general concluſion. *Since* *factitious airs have been ſo freely uſed with ſo little injury, may we not ſafely perſevere till their virtues or want of virtue ſhall be aſcertained?* And however ſtrenuouſly I may have contended for the propriety of the inveſtigation, I have been careful expreſsly to ſubjoin, that *I would not venture to give the ſmalleſt aſſurance of ſucceſs in any one denomination of diſeaſe.* Beſides thoſe that approved and thoſe that unwarily miſrepreſented, there was a third party; and this "not the better portion of the German phyſicians; which ſays Dr. Girtanner, reviled the "author and his project in the coarſeſt terms, "juſt as if it were a crime to try to alleviate the "afflictions of humanity, or to propoſe new "and powerful means againſt incurable diſ-"eaſes." The reader will not be ſurprized that I ſhould have often been amuſed by critics of this deſcription, when he is informed, that ſome of them even tell the public that they incline to believe "that I am *totally* ignorant "what oxygen gas is (*Journal der Erfindungen** St. 17. s. 68.

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* The ſurmiſe aroſe from confounding what I ſay of the reſpiration of *diluted* and *undiluted* oxygen gas.

Opinions, equally honourable to themſelves and to the ſubject of their ſtrictures, have been expreſſed by ſome of thoſe Britiſh literary ruffians who engage by the day, or the week, or the month, to aſſaſſinate literary reputations on account of delinquencies, not literary. This is in the order of motive and conduct. But one cannot help feeling concern when a pains-taking obſerver* gives vent to his ſpleen in ſuch language as the following. Dr. Fordyce (*3d. diſ. on fever. p.p.* 173, 4, 5. after aſſerting that oxygen gas " does not ſenſibly affect the mat-" ter of the body, except that it adds yellow to " the red particles of the blood, which is ſo " altered in the circulation through the body, " that it muſt paſs through the lungs, and from " them again into the other parts of the body, " in order that a man ſhould exiſt" introduces a ſally better calculated to convulſe a circle of pupils, raw from the peſtle and mortar, than to ſatisfy perſons who think. "Whenever, ſays the " Doctor, any new and ſeemingly important " fact has been diſcovered,- - - - - - mankind in " general, and very often even practitioners in " medicine, conceive it muſt be applicable to " ſome medicinal purpoſe. Juſt as an infant, " allured by any thing which glitters in its eye, " applies it to its mouth, ſuppoſing* it muſt " be likewiſe excellent food; ſo infants in " medicine are dazzled with any ſurpriſing " diſcovery, and immediately employ it for " the cure of diſeaſes, not conſidering how " extremely difficult an art medicine is;

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* Dr. Crichton (*Mental derangement*, i. 46) ſays there is ſcarce any treatment of conſumption but has ſhewn "equal, if not ſuperior" powers to a reduced atmoſphere. The repoſe alone, which I have often known follow the uſe of gaſes, ſeems to ſhew that this is a miſtake. I have nowhere ſaid that occaſional ſmall reſpirations of gaſes and vapours have cured, or promiſe to cure, conſumption. And where have they been kept conſtantly applied to diſeaſed lungs? Can Dr. C.'s learning ſupply a ſatisfactory reference to facts of this nature? Dr. C. aſſerts (p. 35) that I have adopted Dr. Girtanner's opinion concerning irritability. This is falſe. In my earlieſt conjectures (*Obſ. on calculus*, p. 264) I proteſted againſt this interpretation of my words: and Dr. C. ſhould have attended to what I have ſince written, ſince he choſe to notice my opinions.—As to Dr. C.'s book, I regret that he had not made himſelf maſter of our beſt writings on pneumatology. To quote from an unknown foreign work, gives in the eyes of ſome readers, an awful air of erudition. For myſelf, I always thought thoſe German magazine ſtories about mad people, fitter for chimney corner goſſiping than for philoſophizing on human ideas and feelings. The uſe made of them by Dr. C. has nothing altered my opinion.

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* The Doctor is, I believe, miſtaken in regard to the infant's motive.

“ how fallacious experiments made in it often “ are, as has been obſerved long ago by Hippo- “ crates, and by what ſlow degrees valuable “ medicines have had their powers inveſtigated; “ how long it was before the effects of the bark “ of cinchona, of mercury, of antimony were “ brought to light, as far as they are already “ known.” Oxygen gas however is not a thing foreign to the actions of life: and it appears much more peeviſh to throw out ſuch inſinuations, than childiſh to ſuppoſe it applicable to “ ſome medicinal purpoſe.” It is by Dr. F.’s confeſſion, neceſſary to exiſtence: Is it not likely then by varying its proportion, that we may vary the mode of exiſtence? We alſo *do* know much more of its effects on the matter of the body than its adding a yellow to the red of the blood. Mr. Lavoiſier’s experiments on reſpiration and thoſe which I have publiſhed, demonſtrate its great power on the actions of the arterial ſyſtem.—As to the reſt, conſidering what an apprenticeſhip mankind have ſerved to the art of making experiments, we may hope to be able to bring out certain reſults ſooner than could be done by the antients, or in thoſe half-barbarous ages, when controverſy ran high concerning antimony, mercury, and bark. And the longer we are likely to be before we arrive at the end propoſed, the ſooner ought we to ſtart, and the moſt briſkly to advance.

In the fourth part of *Conſiderations on airs, p.* 123. and ſq. I have given reaſons for ſuppoſing that living with cows would be ſerviceable in ſome conſumptions. I have frequently ſince recommended the practice, but for the moſt part, as might be expected, in vain. However, in four caſes the experiment was made. In the firſt, the patient's bed was brought to the mouth of a cloſet and encloſed with canvaſs, the cow being brought every evening into the cloſet. During the night the patient had the animal's breath in abundance; but none of thoſe fumes from putrefaction, on which I apprehend the benefit to depend. It was a caſe of the laſt ſtage of conſumption. No effect, good or bad, followed.—In a ſecond caſe, the patient *lived* with three cows, without benefit. The treatment was not conducted under my inſpection; and as the cows were brought into the dwelling-houſe, I am not informed whether the room was carefully cleanſed of the cow-dung and urine. I ſuppoſed this to be a caſe of tubercular conſumption. The third caſe was that of a perſon with black eyes and hair; ſo far advanced, that I was doubtful whether my patient could ſupport the journey from Briſtol to his ſeat near Leeds, where he determined to make the trial. His brother, Mr. W. Greenwood of Leeds, gave me the following account of the reſult. "The remarkable caſe of the lady, gave my brother and his friends hopes of the poſsibility (if not of a cure, at leaſt) of ſuch relief as might protract his time. He therefore proceeded according to the directions in the caſe alluded to, but *not* with the ſame ſucceſs. I am ſorry I cannot give you an accurate ſtatement of the progreſs of the experiment, or of its effects on the patient. The air was of the moſt balmy fragrance, warm, yet refreſhing, its ſmell not unlike that of new-mown hay. The thermometer in a morning (the door having been ſhut all night) was genarally at 90° or above. In the day, freſh air being frequently admitted, it ſtood between 70 and 80. It is to be lamented, he did not make this trial ſooner, for *as it was*, there is no doubt but it not only lengthened his days, but *greatly alleviated* his ſufferings. Had it been begun in an earlier ſtage of the diſorder, it is not to be ſaid how "favourable the event might have proved."—The reſult of the fourth caſe may be ſeen in the ſubjoined ſtatements. Mr. Sandford ſays, "In compliance with your wiſhes and my own inclination, I have procured from Miſs E. Seward, ſome account of the great benefit her late lamented brother, Dr. Seward, experienced by living in a cow-houſe for about ſix weeks previous to his death, and which he was induced to make trial of, in conſequence of the well-authenticated letter on its good effects in pulmonic affection, publiſhed by you in the fourth part of "conſiderations on factitious airs," p. 123. And here it may be neceſſary to remark, that previous to the adoption of this plan, he inhaled various modified airs, though with very inferior ſucceſs to that of living in a cow-houſe. I viſited him twice during the time he principally ſpent in it. The building was conſtructed with large *ſtones* and mortar, *ſuch* as abounded in great plenty in the neighbourhood where he then reſided, after quitting Worceſter. It was built up to the gable end of a brick barn, which formed one of its walls, and a ſloping thatched roof, ſupported by three ſtone walls, of ſufficient dimenſions to contain four cows, conſtituted the building. The height from the ground to the bed-room, was about eight feet, and the ſame from that floor (which was open every other plank) to the higheſt part of the ſloping roof. He ſlept, and remained in the room about ſixteen hours out of the twenty-four, when he walked not more than four or five yards to his mother's houſe, and which was directly oppoſite to the cow-houſe, to eat his dinner, and to enjoy in ſome degree, the ſociety of his affectionate family. But latterly, (as his ſiſter has faithfully related) he found himſelf ſo much more at eaſe when in the cow-houſe (the temperature of which, by a thermometer kept conſtantly there, was about 65 or 66°,) that he could not be prevailed upon to quit it; and in which, he calmly reſigned his valuable life on the 14th of December, 1796. To you who know and eſteemed his worth, I need ſay no more, than lament he had not earlier adopted this plan, by which many of his moſt harraſſing complaints were greatly alleviated."

"The late Doctor Seward ſlept in a cow-room about ſix weeks; he left it by day to enjoy the ſociety of his friends; though he frequently ſaid, that when in the cow-room his reſpiration was eaſy, and when he coughed it was with leſs difficulty, and not ſo frequent, as expectoration immediately followed the effort of coughing.

When in the cow-room, Dr. Seward ſeldom expreſſed by his manner, ſenſations of uneaſy feelings, which towards the termination of his diſeaſe, continually ſhewed themſelves when not in the cow-room, by the repeated deſire of having the air changed, and frequent uſe of a fan. When he took to his bed, he ſaid that he had each day repented leaving the cow-room, and that he ſhould ſurvive it a few days altogether; he lived but four days after this, and was chearful and eaſy except when coughing, till the laſt day. His difficulty of ſwallowing was much abated after he ſtayed in the cow-room, nor was it worse in the evening, as it had uſually been; he took nouriſhment frequently, ſeemingly with much pleaſure, but was then in a very weak ſtate, and in a conſtant perſpiration."

E. S.

Whether the effect experienced in theſe two laſt inſtances do not warrant us, in ſome leſs advanced caſes, to expect a cure from the *continued* application of certain gaſes and vapours, even though it be aſcertained that their occaſional inhalation be of no avail, the editor leaves to the judgment of his readers.

Summary of the late Dr. GEACH'S practice in low fever.

By Mr. S. HAMMICK, Junr.

I do myſelf the honor of communicating to you the following ſketch of treatment purſued by my late learned and truly invaluable friend, Dr. Geach, for ſeveral years paſt, in the typhus, low, nervous, contagious, or putrid fever, (as it is generally called), with great ſucceſs; an account of which treatment, had he fortunately lived, it was his intention to have publiſhed the enſuing ſummer. The reaſon of its being prevented we muſt all moſt ſeriouſly lament; for a treatiſe iſſuing from his pen, on the beneficial effects of calomel and antimony in this diſeaſe, would, no doubt, have been ſo amply ſtored with facts and obſervations, as to have rouzed the univerſal attention of medical men to the ſubject.

The Doctor uſed candidly to confeſs, that he was led to this practice at firſt, about thirty years ſince, whilſt attending the crew of a large Ruſſian ſhip, which had been driven into this port in the greateſt diſtreſs. After encountering ſeveral gales of wind, her people from great fatigue and uncommon exertions, had become very ſickly, and the typhus fever raged with great violence amongſt them, accompanied with ſymptoms of great malignity. He then obſerved that the only men who eſcaped the contagion on board, were men under the influence of mercury, which they had taken for the cure of the lues ven. This fact made great impreſſion on him, and ever ſince that time he had been accuſtomed to give mercury in ſuch fevers, but not with ſuch freedom till the laſt ſeven years of his practice, and for the laſt five years whilſt I had the honor of being an aſſiſtant ſurgeon placed under him in this hoſpital. I have ſeen him preſcribe it, and have preſcribed it myſelf under his own immediate eye and controul, whenever any perſon was ſeized with that fever in the ſurgical wards of this hoſpital; and as I always attended him during that time in his viſits of the wards, the number of caſes has been conſiderable: and I have alſo ſeen it very ſucceſsfully adminiſtered in ſome very alarming caſes of typhus gravior among the poorer claſs of inhabitants of Plymouth Dock, and Stonehouſe, whom humanity induced him to visit in thoſe places, and to whom he had the goodneſs to take me, in order to be thoroughly convinced of the efficacy of this remedy, and thereby induced, from actual obſervation, to give this medicine with confidence in my future practice; for the doctor thought this plan only wanted publicity to obtain a preference to thoſe uſually employed in ſuch diſeaſes.

The following is only an imperfect outline of the plan, but even as ſuch, I truſt it may not be deemed unworthy of your peruſal—imperfect, as I have not had acceſs to his notes and obſervations, but at the ſame time I pledge myſelf for its faithfulneſs.

Whenever the doctor was called to a perſon labouring under ſymptoms of typhus fever (if within two or three days of its firſt attack) he uſed conſtantly to preſcribe fourteen or ſixteen grains of ipecacuanha, aſſiſting its operation with chamomile tea; three hours after the ceſſation of the vomiting (if the patient was delicate), a bolus of five grains of calomel, with a ſcruple of rhubarb was given, but if the patient was of a ſtrong habit, a ſcruple of jalap, with eight or ten grains of calomel, were adminiſtered. If evacuations were not thus produced within eight or ten hours, caſtor oil, or ſome other laxative, were given occaſionally till the deſired effect had taken place. The windows of the room were opened in ſuch a manner, that the room was kept perfectly cold, without ſubjecting the patient to a current of air ; the bed-curtains nearly all withdrawn, ſo that free circulation was admitted, even in winter ; taking care to have (where it could be procured), frequent changes of linen. After the ſtools, the following boluses were immediately ordered :— calomel ~~eight~~ three grains, pulv. antimonial. four grains, conſ. cynoſb. q. ſ. ut f. bol. to be taken every ſix hours when the ſymptoms were ſlight, but when the caſe was very urgent, or he had not been called in till the fever had made ſome progreſs, then the above quantity was given *every four, three,* or even *every two hours,* permitting weak lemonade, tamarind, or cream of tartar water to be taken for the common drink. If the fever ſtill went on, and the patient's ſtrength became exhauſted, a little port wine diluted with water was allowed ; uſual quantity half a pint, ſeldom or never exceeding one pint in twenty-four hours. To any perſon unaccuſtomed to give theſe boluſes, diarrhœa, ptyaliſm, or vomiting, would naturally ſuggeſt themſelves as the inevitable conſequences in almoſt every caſe of their exhibition; but the fact, in a multiplicity of inſtances, directly proves the reverſe; for in general we are obliged to order a little caſtor oil, rhubarb, with kali ppt. or an electuary, made of equal parts of cream of tartar and conſerv. cynoſb. Ptyaliſm has ſeldom, as I have before ſaid, followed their uſe, notwithſtanding they have been continued to ſome patients every *three* hours, for eighteen or twenty days: but when they did affect the ſalivary glands, the cure was always certain and expeditious after that event, appearing to check immediately the progreſs of the diſorder. When diarrhœa ſupervened, the doctor was cautious how he checked that diſcharge, never attempting it, unleſs the patient was very feeble or low; for in ſeveral inſtances where numerous ſtools have been procured, the patients have found themſelves relieved of a delirium which had been on them for three or four days before, but when the diarrhœa continued profuſe, exhauſting the patients ſtrength, then means were employed for its removal, commonly a ſcruple of conf. opiat. or an ounce of poppy ſyrup ſufficed: if they did not, half a grain or a grain of opium was combined with the calomel and antimony, but ſeldom were we neceſſitated to ſeek the aſſiſtance of opium, and in no other way did the Doctor ever adminiſter opium in this diſeaſe. Vomiting when excited, was commonly allayed by the ſaline mixture in the ſtate of effervefcence; when this ſymptom much harraſſed the patient, the antimonial powder was reduced from four to two grains: this was the ſyſtem purſued throughout the whole of the ſtages of this fever, never adminiſtering any other medicine, unleſs any extraordinary occurrence took place; therefore the whole dependance for a cure may be clearly perceived to be entruſted to the calomel and antimony. In ſome few caſes, when delirium was great and and the head much affected, a bliſter was applied to the nape of the neck; as ſoon as ſigns of amendment appeared, the boluſes were diſcontinued, *and not till then*; a little mutton broth or jelleys were allowed and a decoction of bark with balſ. tolu was given; but the bark in ſubſtance was never given by the doctor; for the bad effects of it in this form, when exhibited to weak ſtomachs, far outweighed, in his opinion, any good it ever produced. It is well worthy of remark, that in all thoſe caſes where the ſymptoms were very urgent, and the putrid appearances more apparent, that there the boluſes ſcarcely ever were obſerved either to ruffle the bowels or ſtomach.

Now, Sir, after the above ſtatement, it may be expected that we have ſome theory to defend this innovation of practice, and that I ought, after troubling you in this manner, to attempt accounting for the modus operandi of this medicine, which, in a variety of inſtances, I freely confeſs myſelf incompetent to do, and even were I capable, unwilling to do; for in theory we may be overthrown in various ways, but in the above account we never can; for who can overthrow us when truth is our foundation? Feel aſſured, ſir, this was the mode of practice purſued by Dr. Geach in thoſe caſes, not only in this hoſpital as firſt Surgeon, but alſo in a moſt extenſive range of private practice, and of courſe nothing but the ſucceſs attending it, could have induced him to perſevere in a treatment ſo widely different from that purſued by other practitioners. Should you be diſpoſed to propoſe any queſtions to me more explicit of it, I ſhall moſt readily anſwer them, and conſider myſelf at the ſame time, honored by the application. Or ſhould you deem this letter worthy of being ſhewn to any of your medical friends, the freeſt permiſſion accompanies it.—I am, &c.

STEPHEN HAMMICK, Junr.

To Dr. BEDDOES.

P. S. I beg leave to ſtate, that my father, about five years ſince (in the abſence of the phyſician) at the recommendation of Dr. Geach, purſued the above plan, with very great ſucceſs, in a number of caſes of typhus gravior, received into this hoſpital from his Majeſty's ſhip Squirrel, on board of which ſhip, the fever had been ſo violent, that the Board of Admiralty gave an order for deſtroying the bedding and cloaths of the men, ſupplying them anew at Government's expence; and alſo, that my friend, Mr. John Fryer, Viſiting Aſſiſtant Diſpenſer at this Hoſpital, who, when a fever of the worſt ſpecies of typhus was raging among the french priſoners confined at Mill-Priſon, Plymouth, about three years ſince, and at a time when moſt of the Aſſiſtants there employed, were confined by the fever, nobly and humanely volunteered his ſervices, found that the calomel and antimony was very far ſuperior to every other practice. Of their teſtimonies, it was the Dr's. intention to have availed himſelf in the purpoſed pamphlet.

Royal Hoſpital, Plymouth, Oct. 21, 1798.

To Dr. BEDDOES.

Since the preceding communication came to hand, the Editor has received from Mr. H. a note to the following purport. "Since I did myſelf the honor of tranſmitting you the outlines of Dr. Geach's treatment of typhus, I have found that the Dr.'s unfiniſhed manuſcript on that practice, hath fallen into the hands of a profeſſional and intimate friend of mine, Mr. Knighton, of Plymouth-Dock, one of the late Dr.'s boſom friends; which I have preſſed him to publiſh, and have the ſatisfaction of ſaying, he intends it, together with ſome of his own practical obſervations. The above notice of Mr. Knighton's deſign, I ſhould wiſh you to make known to the public, by inſerting it in the publication in which my ſketch of that practice will appear."

Extracts of Letters, from Mr. COOKE, *Apothecary, Glouceſter.*

As I have had frequent opportunities of making enquiries upon the very important ſubject of Dr. Jenner's ingenious treatiſe, during an extenſive inoculation in various parts of this country (within the laſt two months) I wiſh to communicate to you a recent caſe (well authenticated) which I think, will ſufficiently prove, that the cow-pox is not an infallible or invariable preventative againſt acquiring the ſmall pox; "*unleſs the cow-pox is only a poſitive preventative*
"*againſt acquiring the ſmall pox in a natural way,*
"*or unleſs natural changes in conſtitution from*
"*certain periods of life, may have any influence in*
"*diverſifying the propenſity to particular diſeaſes.*

December 5, 1798, I inoculated Mrs. Carter, of Longney, in this County, in the left arm, with variolous matter taken from a patient having the diſtinct ſmall pox. The arm did not appear much inflamed for ſeveral days, but upon the ſeventh, ſuppuration took place. She ſickened upon the ninth; the eruption commenced upon the twelfth; ſhe had rather a burthen of puſtules, is now recovering without any variation from the common courſe of inoculated ſmall pox.

When Mrs. Carter applied to me, reſpecting the ſmall pox, it was to inoculate her ſon, with ſome other patients in the ſame place, but ſhe ſaid, "as ſhe was formerly moſt violently affected by the cow-pox, and had frequently "been with perſons who were infected with the "ſmall pox, both in the natural way and from "inoculation, without catching it, ſhe thought "it unneceſſary to be inoculated herſelf, particularly as ſhe had read in the newſpaper "that perſons once affected with the cow-pox, "could never have the ſmall pox" I remonſtrated and adviſed that ſhe ſhould be inoculated, obſerving, "that provided ſhe did not have the "diſeaſe, no harm could poſſibly enſue from "being inoculated;" ſhe therefore conſented; and the ſequel proved that ſhe either never had the proper cow-pox of Dr. Jenner, or that the cow-pox is not a preventative (invariably) againſt the ſmall pox. Mrs. Carter is fifty years of age, had ceaſed to menſtruate for ſeveral years, is a very healthy woman, and gave the following account of her having the cow-pox.—When eighteen years of age, ſhe lived in a dairy farm, in Longney; at that time the cows were affected with chopped and ſore teats, all the ſervants who ſtripped theſe cows, had inflammation and boils upon their hands. She was ſo ill with fever, and theſe boils, that ſhe could not work for a week; her hands and arms were dreadfully ſwelled, and ſhe kept her bed for two days. In this ſtate, ſhe applied to Mr. Cork, who then was in practice at Frampton, in this County, he told her, "ſhe had the cow-"pox very bad, and that it was a diſeaſe the "neareſt to the ſmall pox that could be." Her fellow-ſervants all recovered without medical aſſiſtance, by the uſe of a frequent application of bole armenia, but it did not ſucceed with her, for Mr. Cork, upon ſeeing her, made inciſion in various places about her arms and hands, and ſhe has evidently the marks remaining at this time. Mrs. Carter has lived in Longney ever ſince, and many of the neighbours recollect her being ill with the cow-pox.

I have only to add, that as I paſs this woman's door every other day, if I have omitted any enquiries, I will make what others you may think neceſſary, ſince it will give me infinite ſatisfaction to ferret out (within the boundaries of my practice) every circumſtance that can tend to eſtabliſh or confute Dr. Jenner's opinion "that cow-pox moſt compleatly deſtroys "the propenſity to, (or the power of infecting "the human body with) the ſmall pox."------

In order to ſatisfy my own mind, that the caſe of Mrs. Carter could admit of no doubt, I have uſed every endeavour to obtain the beſt information in this neighbourhood, upon that diſeaſe, termed the cow-pox, and upon its effects when communicated to the human ſubject. I now beg to ſubmit to your peruſal, the reſult of my enquiries, and I think it will ſufficiently prove, that we ought to deſiſt from every attempt to eradicate the ſmall pox, by inoculating with cow-pox matter, ſince the utmoſt to be expected, is the procraſtination of a diſeaſe more dreaded than any other to which we are liable.—Beſides, we are told, that the introduction of this animal diſeaſe into the human ſyſtem, although never fatal and ſhorter in its progreſs, is more violent in its operation, than the introduction of the ſmall pox. Why then prefer this diſeaſe of brutes, to one which we are well aſſured is peculiar to man (and by no means fatal, when the patient is not unhealthy or under dentition)? The former is probably only a local diſeaſe in the brute, the latter, we know to be a conſtitutional diſeaſe in man. An indefinite period of time can only determine the effects of the former, (and it ſeems probable, that whatever its influence reſpecting the ſmall pox may be, it is in a certain time entirely deſtroyed) whereas, the latter diſeaſe may be intended for certain uſeful purpoſes in the human œconomy. If the introduction of diſeaſes peculiar to the brute creation, or animal poiſons could either prevent or eradicate the virulence of human diſeaſes, which are as yet ungovernable by any known remedies, there could not be a greater acquiſition to medical knowledge. But ſince it will appear, that cow-pox cannot have any ſpecific effect in preventing the ſmall pox, the public mind ought to be at once relieved from its preſent ſuſpenſe, by every ſatisfactory evidence that can be brought forward upon this ſubject.—I truſt I ſhall be acquitted of any other reaſon for communicating the following facts, than my reſpect for that profeſſion, of which I am proud to be a member, and from conſidering it my duty, to report what I have heard or ſeen upon a ſubject, which at preſent ſo much agitates the public mind.——I am, &c.

CHARLES COOKE.

Mr. Clayton, living in Gloceſter, ſays, that he is in the habit of attending the cows, at the dairy farms, in moſt parts of this County, for ten miles round Glocceſter: that the conſtitutional diſeaſes of cows are by no means numerous (many of which are anomalous and rarely to be met with,) but the moſt frequent to be obſerved, are the following:—The red water, the blain, the yellows, and the murrain; theſe are all the diſeaſes that can with propriety be nominated conſtitutional, becauſe the fevers to which cows are liable, are generally ſymptomatic, and ariſing from local pain: that the local diſeaſes are very few, of which, the lough, ſwellings of the udder, and cow-pox, are nearly the whole to which a name can be given; the two former are moſt common, the latter rarely to be ſeen, excepting in the ſpring and ſummer ſeaſons: that the cow-pox begins with white ſpecks upon the cow's teats, which in proceſs of time, ulcerate, and if not ſtopped, extend over the whole ſurface of the teats, giving the cow excruciating pain:—that if this diſeaſe is ſuffered to continue for ſome time, it degenerates into ulcers, exuding a malignant and highly corroſive matter; but this generally ariſes from neglect in the incipient ſtate of the diſeaſe, or ſome other cauſe he cannot explain :—that this diſeaſe has not a regular proceſs of commencing and terminating without a remedy, becauſe, if not attended to, it would end in a mortification of the teats, and probably death of the animal :—that this diſeaſe may ariſe from any cauſe irritating or excoriating the teats, but that the teats are often chapped without the cow-pox ſucceeding. In chaps of the teats, they generally ſwell; in the cow-pox the teats ſeldom ſwell at all, but are gradually deſtroyed by ulceration :—that this diſeaſe firſt breaks out upon one cow, and is communicated by the milkers to the whole herd, but if one perſon was confined to ſtrip the cow having this diſeaſe, it would go no farther :—that the cow-pox is a local diſeaſe, *and is invariably cured* by local remedies :—that he never knew this diſeaſe extend itſelf in the higheſt degree to the udder, unleſs mortification had enſued, and that he can at all times cure the cow-pox in eight or nine days, by his uſual local remedies :—that he is converſant with the diſeaſes of the horſe, and extenſively employed, particularly in curing the greaſe :—

that he cannot recollect ever to have had horſes with the greaſe and cows with the cow-pox, under cure at the ſame time, and at the ſame farm :—that he is very certain, he has frequently had cows with the cow-pox, where no horſes whatever have been kept :—that he conſiders the greaſe as a name, having great latitude in the diſeaſes of horſes, becauſe, ſometimes the cure of it may be effected merely by topical remedies, and at other times, it is only to be compleated by internal remedies :—that he does not conſider the greaſe an infectious diſeaſe among horſes, ſince greaſy horſes and horſes in perfect health, frequently ſtand in ſtables together indiſcriminately, without infecting each other; and although it is probable if the diſcharge from greaſe was to be applied in its moſt acrid ſtate to the heels of a ſound horſe, it would inflame and excoriate them, yet it would not produce the greaſe :—that the greaſe is moſt prevalent in the winter, at which time, he has never known the cow-pox to occur, and therefore, cannot think it at all probable, that the greaſe can have the leaſt influence in producing the cow-pox. There is little variation from this account, in the information I have obtained from ſome of the moſt reſpectable dairy farmers in this neighbourhood. Thoſe who have ſeen the cow-pox among their domeſtics, all agree, that if they have been ſoon afterwards inoculated for the ſmall pox, they have had the diſeaſe very ſlightly, but ſince the late general inoculation, are as fully ſatisfied that many have had the ſmall pox in a more decided manner, who ſome years before, had the cow-pox very ſeverely.

Converſing with Mr. ——, an eminent and very extenſive practitioner in this County, upon the cow-pox, he ſaid, that he was confident every perſon who previouſly had had the cow-pox, as certainly had the ſmall pox afterwards if they were inoculated, but in a much ſlighter degree, and where any eruption ſucceeded, it was without maturation :—that the arm of a perſon previouſly having the cow-pox, aſſumed a ſingular appearance while under the action of ſmall pox from inoculation :—that the fluid in the arm inoculated, ariſing from the previous inflammation, was merely lymph, as the inciſion never ſuppurates :*—that he has frequently inoculated other patients with this lymph, with equal ſucceſs, as if he had uſed the natural variolous matter :—that he never recollects to have ſeen a patient having the cow-pox, but after having inoculated one or two perſons, who ſaid they had previouſly had the cow-pox, he could always tell, whether any of his patients under inoculation, had had that diſeaſe, from the progreſs and ſingular appearances of the arm :—that during a practice of forty years, the number of thoſe having previouſly the cow-pox, does not, he thinks, amount to more than ſixty, although he has inoculated many thouſands for the ſmall pox :—that he has, by way of experiment, a ſecond and third time inoculated perſons, who having firſt had the cow-pox, had been afterward ſucceſsfully inoculated for the ſmall pox; and no further effect was produced than upon the arm of other perſons who before had the ſmall pox, either from inoculation or in the natural way. Mr. Beach of Hardwick, informed me, ſince writing the above, that Farmer Tombs, of Hasfield, near Glocester, who had the cow-pox many years ago, caught the ſmall pox in coming to Glocester market, and died of it, in December laſt. He ſays, that ſeveral of his neighbours can teſtify Mr. Tombs had the cow-pox many years ago, but was rather cautious in going to any place where he knew the ſmall pox particularly prevalent, although he never would conſent to be inoculated, thinking he was ſafe :—that lately, Mr. Tombs, was not at all afraid even to enter an houſe where he knew the ſmall pox to be, and conſequently at length, fell a victim to that diſeaſe.

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* I will take the firſt opportunity of aſcertaining, whether the ſame appearances and progreſs of inflammation would occur, provided a patient, who before had the cow-pox, was inoculated in the thigh, for the ſmall-pox.

CHARLES COOKE.

Letter from Mr. THORNTON, *Surgeon, Stroud, dated Feb.* 7, 1799.

In conſequence of your letter to me of the 1ſt. inſt. I ſend you the following account of my experiments relating to the cow-pox. On the 1ſt. of December, 1798, being informed that the cows on Stonehouſe-Farm had the cow-pox, and that a man who milked them was infected with the diſeaſe, I called on him that day, and found him with puſtules on his hands and fingers, which had made their appearance four days before. The patient had not had the ſmall-pox: the ſymptoms he experienced previous to the eruption (he told me) were pain in his head and in the axillæ, with frequent cold ſhiverings, fever and debility; on the ſecond day the cow-pox broke out, which terminated his complaints. I immediately procured ſome matter from a purulent pock, which was the only one that was not degenerated into a ſordid and painful ulcer. I, that evening, went to Stafford's-Mill, and inoculated Mr. Stanton and and four of his children, the eldeſt was ten years old, the youngeſt about ten months. On the third day, all their arms appeared to be under the influence of a very active virus; the arm of the youngeſt child was affected with a kind of eryſipelatous inflammation, the ſize of a half-crown piece, without any elevation of the cuticle, it was half an inch above the place where the matter was inſerted, with which it did not ſeem to be in the leaſt connected; on the fourth day, the inflammatory appearances of the three eldeſt were increaſed; the youngeſt child's arm had loſt that efflorcſcence, but about the puncture the redneſs was increaſed: Mr. Stanton's was evidently on the decline, and from this time gradually died away. On the ſixth day, the ſkin round the inciſions of the children's arms was conſiderably elevated, and contained a limpid fluid. The inflammation in each kept on till the fourteenth day, when the punctures began to be covered with a cruſt of conſiderable thickneſs, from which an ichorous matter continued to diſcharge for ſeveral days, without any diminution of the ſurrounding inflammation. About the twentieth, the ſcabs fell off, and the inflammatory appearances ſubſided. During the whole proceſs, there was no commotion excited in the ſyſtem, nor the leaſt pain or uneaſineſs perceived in the axilla of either.

From the long continued local excitement, I began to entertain a hope that the virus might imperceptibly have crept into the habit, and proved a ſecurity againſt the variolous infection. To relieve my own doubts, and to enſure the ſafety of my patients, I had immediate recourſe to the introduction of the ſmall-pox matter. All the children received the infection, and paſſed through the different ſtages of the diſeaſe in the uſual ſlight manner. Mr. Stanton's conſtitution reſiſted my repeated attempts to communicate it to him. I therefore conclude, as he ſpent the early part of his life in London, that he might have had the ſmall-pox ſlightly during that period.

Concerning Mr. Colborne's children, I have received authentic information " that three of " them were inoculated with cow-pox matter, " together with a ſervant-man ; two of the chil" dren ſuffered ſeverely from violent inflam" mation and alarming ulcerations in their " arms. They were all inoculated afterwards " with the ſmall-pox matter ; the two whoſe " arms had been ſo dreadfully affected, did not " take the ſmall-pox, the others received it."

If you ſhould think this communication deſerving a place in your intended publication, you may inſert it.

EDWARD THORNTON.

Dr. BEDDOES.

P. S: Some caſes of cow-pox have lately occurred in this neighbourhood; if on further inveſtigation they appear to throw any light on the ſubject, which they bid fair to do, I will take the liberty to let you know the reſult of my inquiries; they at preſent appear to operate againſt Dr Jenner's doctrine of ſecurity.

Anſwers to Mr. (now Dr.) Adams's Queries, concerning the Sivens.

1ſt. If the diſeaſe ever appears without producing a local effect on the part where it may be ſuppoſed to be received?

The diſeaſe almoſt always begins with an inflammation on the velum pendulum palati, and uvula, and afterwards on the tonſils, of a dark red colour, which is ſucceeded in one or two days, or ſometimes ſo late as ſix or eight, by ſmall pimples or veſicles; which leave ulcers, with a white lardaceous ſurface, and red abrupt edges. Theſe ſpread, and after ſome time, dark red ſpots which fall out into ulcers, appear about the perinæum and anus. Very frequently fungous excreſcences, ariſe round the anus, inſtead of the red ſpots; theſe gradually increaſe and ulcerate.—I have been aſſured by ſeveral patients, that their ſore throat had been very ſlight, and ſoon went off, without uſing any remedy, but was ſucceeded by the appearances juſt mentioned. In theſe caſes, I am inclined to believe, that only a ſlight local inflammation was excited by the virus as it entered the ſyſtem; in the ſame manner as we ſometimes meet with a bubo, as the firſt ſymptom of lues, without any preceding chancre. It is perfectly aſcertained, that the breath of people labouring under the ſore throat, is loaded with infection, and communicates the diſeaſe, without the contact of ulcers.

2d. If there is any, and what difference, between the primary and conſtitutional ulcers?

The primary ulcers, in the throat, inſide of the cheeks, or lips, (which laſt are frequent in infants, probably from the infected nurſe kiſſing them) have always a white ſurface, with red thickened edges. The ſecondary ulcers, on the perinæum, belly, thighs, &c. have the ſame appearance at firſt, but after having continued long, they are often of a bright florid red, like cancerous ſores; the diſcharge is thin and acrid, and they are then much more difficult to cure. This change in old ulcers from ſivens, I am inclined to think, is the natural progreſs of the diſeaſe, and not merely the effect of ſtimulating applications.

3d. Whether mercury cures the diſeaſe

without producing thoſe effects on the ſyſtem, which we find neceſſary in the cure of inveterate chancres?

I am convinced from abundant experience, that a leſs quantity of mercury will cure this diſeaſe, than is neceſſary in the common lues venerea. The ſublimate is the preparation that cures it moſt ſpeedily; and even in a few days after patients begin the uſe of it, the ulcers put on an healing appearance. It is neceſſary to continue the uſe of mercury for at leaſt a fortnight or three weeks, after all the ſymptoms are removed, otherwiſe the diſeaſe will infallibly appear again in a few weeks or months. This holds true, whatever preparation is employed, whether ſublimate, unction, or any other form. Salivation is never neceſſary; patients may generally be allowed to go about.

4th. If the primary ulcers are cured before the conſtitutional ulcers have appeared, whether the latter ever ſhew themſelves without the recurrence of the original ulcers?

Nothing is more frequent, when the mercury is diſcontinued too ſoon after the healing of the primary ulcer.

5th. Whether in caſes of phagedena, the parts uſually heal by granulation, or ſkin over without the loſt ſubſtance being renewed ?

There is certainly ſome ſmall part of the cavity of each ulcer filled up by granulation ; but much more of the cure depends on the waſting of the ſurrounding parts.

6th. Whether the diſeaſe is ever cured by the uninterrupted efforts of the conſtitution ?

I am convinced it never is.

7th. If this be the caſe with phagedenic ulcers, whether if the funguſes can be deſtroyed by applying eſcarotics as low as the ſound part, the diſeaſe will ceaſe in that part, and healthy granulations riſe, no other remedy being uſed at the ſame time ?

I believe it is poſſible ſometimes to remove an ulcer or fungus in this manner ; but the attempt will fail ten times for once that it ſucceeds ; and even when it has ſucceeded, the diſeaſe will break out again in the ſame, or ſome other part, in a few weeks.

8th. Whether mercury ever fails to effect a cure ?

I believe it never does, except in thoſe deplorable caſes, where from the long continuance of the diſeaſe, hectic ſymptoms have come on, and the conſtitution is ſo broken down, as not to be able to bear the remedy; preciſely as happens in the common lues venerea.

The ſivens certainly affects the mere ſurface of the body, more than the common lues. Buboes almoſt never occur in ſivens. The bones of the noſe and jaws are often deſtroyed by it, but I never knew any of the other bones affected; the patient dies hectic, from the very extenſive ulcerations, before this could take place. JAMES PATERSON.

Air, July 28, 1798.

Extract of a Letter from Dr. Paterſon.

" It is about fifteen years ſince I read Dr. Gilchriſt's account of the yaws,* in the Edinburgh Phyſical and Literary Eſſays, and I have not the book at hand; but if I do not forget, he conſiders the yaws, not as a particular ſpecies, or modification, of lues venerea, but as exactly and identically that diſeaſe. This I believe to be an error, and I beg leave to ſtate my reaſons for thinking ſo.

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* A country name for ſivens. ED.

1ſt. Lues venerea was common in this country long before the yaws appeared. It is perfectly aſcertained, that the latter diſeaſe was introduced into Airſhire about the year 1745, by people who came from Dumfries to buy cattle. It was brought to Dumfries by a party of ſoldiers who had been ſtationed in the north Highlands; and the tradition there is, that it was imported or produced by the ſoldiers of Oliver Cromwell, labouring under lues venerea. Ever ſince its firſt appearance, it has prevailed in a greater or leſs degree, in the different places, at different times; ſometimes abating ſo much, both in virulence and in frequency of occurrence, as to give hopes that it would entirely wear out; then breaking out again with greater violence, generally in the harveſt ſeaſon, and ſpreading over ſeveral pariſhes. I have often attempted to trace the infection to ſome perſon affected with lues venerea, but never with ſucceſs. I am convinced therefore, that it always ariſes from its own ſpecific poiſon; whatever way that may have been originally generated. Some medical people in this country, imagine it a combination of the itch and lues venerea; while others believe it produced from the angina maligna and lues. With regard to its firſt production I know nothing; but I believe it is, like the ſmall-pox and other ſimilar diſeaſes, never produced now de novo. The origin of theſe poiſons is a very obſcure ſubject, not likely ever to be much elucidated. Latet cauſa, vis eſt notiſſima.

2d. The yaws is undoubtedly much more infectious than the common lues; for it ſeldom gets into a family without infecting every perſon in it, and frequently ſpreads rapidly over a village. If the common lues were to ſpread in a ſimilar manner, its progreſs in all large towns, would be truly dreadful.

3d. The yaws is certainly a more cutaneous affection than the common lues; for it almoſt never affects the large hard bones, and very ſeldom indeed occaſions buboes. I admit, that this may be partly explained from the different manner in which the virus is received, but I think not altogether. I look upon it as an aſcertained fact, that, of all the preparations of mercury, the corroſive ſublimate is the beſt adapted for the cure of ſibbens; that is, it cures it more ſpeedily, and with equal certainty with any other mercurial preparation. This probably ariſes from the ſibbens affecting the ſurface of the body, more than the common lues.

4th. The yaws is more eaſily cured than the ordinary form of lues; for a much leſs quantity of mercury removes blotches and extenſive ulcers, than is required in lues contracted in the ordinary manner. I have had abundant proof of this; but I acknowledge that the diſeaſe is very apt to return, from the patient's leaving off the remedy too ſoon. It is always neceſſary to continue it for ſome weeks after the ſymptoms have diſappeared. Sauvages mentions a ſpecies of ſyphilis, which he calls ſyphilis indica, his third ſpecies: he ſays it is very infectious, but eaſier to cure than the other ſpecies; it appears ſimilar to the yaws of this country.

Theſe, Sir, are the only obſervations which occur to me at preſent, as worth communicating to you, on this diſeaſe. I have written them chiefly with a view to ſhew my inclination to aid you in your uſeful inveſtigations." *J. P.*

On reference to Gilchriſt, I find him aſſerting, that the ſivens was " ſoon diſcovered to be of the venereal kind," that " if not the " ſame, it has an exact reſemblance both in " its ſymptons and cure", and that, " the ſolu-" tion of corroſive ſublimate has often been " tried, allowing the patient to go abroad, but " not with the deſired effect." (Edinburgh " Eſſays, III. 1771.)—I have no new information on the *ſiphylis indica* of Sauvages; but I will notice an academical publication, which came out four years ago, in hopes that ſome of our literary countrymen in Aſia, may be induced to examine into the validity of the author's aſſertions. It is J. G. Klein, *Tranquebarra—Dani ſpec. inaug. de morbi venerei curatione in India orientali Hafniae.* 1795. Mr.

Klein contends that the venereal diſeaſe was known in the Eaſt Indies long before the diſcovery of the Weſt Indies. The reaſons aſſigned by the author are theſe: The antient medical writers, Sangaraſiar and Aleſſianambi, ſpeak of this diſeaſe, and its cure by quickſilver, nine hundred and ſeventy years ago. Other writers ſtill more antient, Tanmandari, Achaſtyer, Tirumuler Poger, ſpeak of the hiſtory, ſymptoms, and mercurial treatment of lues venerea. The name *Moecha Wiadi*, is a Tamul word, and occurs in thoſe antient authors. It is therefore unqueſtionably one thouſand years ſince thoſe works were compoſed.—It has been rendered probable by Henſler, Gruner, and Sprengel, in oppoſition to Aſtruc and Girtanner, that the venereal diſeaſe was not brought from America to Europe. But as in the caſe of the writers before the return of Columbus, ſo with reſpect to the Oriental writers abovenamed; the doubt is, whether their deſcription is perfectly ſatisfactory. This doubt the members of the literary and philoſophical ſociety at Calcutta, may be able to ſolve. EDITOR.

Caſe by Mr. G. Viſe, Stilton, Hunts. dated Feb. 1, 1799.

ANN HIGBY, of Yaxley, in the County of Hunts. aged 27 years, was ſoon after the delivery of her firſt child, attacked with a quartan ague; it continued nine years, although every medical aſſiſtance was uſed. During all this time, ſhe frequently complained of a pain on the left ſide. About the expiration of this time, the ague left her for twelve months: it returned from taking cold, and continued for ten years. On the intermitting days her health and ſpirits were very good during the whole of the time.—About the age of 48, her catamenia left her, and from that time ſhe loſt her ague. The pain and enlargement of the ſide began to increaſe, and from that time her body began to fill. In Feb. 1788, ſhe applied to me for the firſt time: after giving me the above hiſtory, I examined her body, and made no heſitation in declaring it contained a fluid. Upon tracing the courſe of the ſide, I evidently diſcovered enlargement of the ſpleen. I told her I would try what medicine could do for her, which if it did not ſucceed, I ſtrongly recommended paracenteſis. I uſed the digitalis infuſion, in the manner ordered by *Whithering*, without the deſired effect. On the March following, I performed paracenteſis, and drew off two gallons and five pints of dark-coloured water, nearly the color of coffee grounds. From that time nothing very particular occurred. I performed the operation ten times, from March 1788, to April 15, 1796. About the end of October, 1796, ſhe applied to me to perform the operation again, but from ſome cauſe which I do not recollect, it was put off for a few days. Nov. 5, a few days after her application to me, I was ſent for, the meſſenger informed me ſhe had fallen down forwards on a brick floor, and had never ceaſed vomiting ſince the accident. I did not ſee her until the next morning, Nov. 6, when I was informed ſhe had carried her bread to the baker's, in coming out of whoſe houſe, her feet ſlipped, and ſhe fell flat upon her belly; ſhe was immediately deprived of the uſe of her legs, and was brought home in a chair. Vomiting immediately followed. Upon enquiring, I found ſhe had not made water, or had a ſtool, ſince the accident, every thing was rejected by the ſtomach. I immediately ordered her a common enema, with an ounce of ſalts, and the following medicine:—Conſ. roſar. unc: unam; elix. vitriol. acid drachm. un; tinct. opii gtt triginta; aq. cinnam. unc: unam; aq. menthæ q. ſ. f. miſt. unciar: sex capt coc. h. duo ſeta quaque hora et repetat urgente vomitu. At the ſame time, I ordered the body to be bathed with warm water, afterwards rubbing it all over with the volatile liniment. On the 7th, enema repeated, and liniment continued, with the fomentation. On the 8th, enema repeated, miſt: ſalinos: cum magneſia: alb: continuing the fomentation and liniment as before. On the 9th the ſame, 10th the ſame. The enemas were all rejected without fæces, and the vomiting continued very violent indeed. I frequently examined her body, and found it diminiſhed in ſize every day, and I think during the ſix days, ſhe vomited upwards of two gallons of fluid, nearly the colour of what

I uſed to draw off from her in paracenteſis. 11th. I ordered the following pills: pil. ex coloc. cum aloe drach: un. pilul: xii. She took the whole of theſe pills before a ſtool was procured, during all which time, ſhe never made water. After the operation of theſe pills, ſhe made water, and recovered her ſtrength very faſt, and informs me ſhe never had her health ſo well theſe thirty years. I called upon her ſince your laſt letter of Jan. 27th 1799, and found her in good ſpirits, but informed me ſhe has had ſome little return of her ague within this laſt month, but has never had the leaſt ſymptoms of dropſical complaints ſince Nov. 5, 1796, and has been perfectly well.

JOSEPH VISE.

To Dr. Beddoes.

P.S. I have had one venereal caſe of my own; ſo has one of my friends; in both of which the nitrous acid did wonders—both were ſecondary.

On the uſe of Nitrous Acid *in reſtraining ſickneſs*, by the Editor.

I have ſcarcely found any medicine ſo certain in its effect, as nitrous acid in relieving various ſickneſſes. Its power as compared with that of other acids, I cannot aſſign. The following inſtances, of which none but 4 & 5 are ſingle in their kind, will ſuffice for the information of readers who may chooſe to put my aſſertion to the proof.—1. A lady of weak conſtitution, light hair and eyes, not chlorotic, liable to frequent inappetence and ſickneſs at riſing, was directed to take from ſix to ten drops of nitrous acid in water with ſugar. On a great variety of occaſions it never failed to remove the ſickneſs. A repetition of the doſe was ſometimes neceſſary. 2. A boy with dark hair and eyes, ſubject to frequent ſickneſs and bilious vomiting from various cauſes, as fatigue, a ſmall exceſs in eating, taking cold—whoſe digeſtive organs in ſhort, ſeem to ſympathize with every part of the ſyſtem in diſorder—has always been certainly relieved by a few drops of nitrous acid. 3. A perſon attacked in autumn with bilious vomiting and purging, had the ſymptoms immediately mitigated by nitrous acid, diluted and ſweetened. Repetition of the acid ſoon carried off the attack. The ſame plan has equally ſucceeded in other caſes of cholera—4. A gentleman, after eating freely of cheeſe much decayed, was ſeized with ſickneſs and bilious vomiting. A doſe of nitrous acid inſtantly relieved the ſickneſs, and a ſecond carried it off.—5. A lady in whoſe right hypochondrium a moveable hard tumour can be felt, complained of loſs of appetite, weakneſs and emaciation, with pain in the ſtomach ſo violent, as to oblige her to take one hundred drops of laudanum, once or twice a week. The daily uſe of nitrous acid from twenty to forty drops, diluted and ſweetened, much increaſed the appetite at firſt, and has kept the ſtomach for ſome weeks free from pain.—6. A gentleman, who had lived freely, and ſpent ſome time in a tropical country, complained of obtuſe pain in the right hypochondrium, and occaſionally of more acute pain in the right ſhoulder. His countenance was ſallow, he coughed hard, with expectoration of a little frothy mucus, ſuch as are vulgarly called *ſixpences*—pulſe 120—fæces not ſufficiently coloured—countenance ſallow. Suppoſing the liver to be diſeaſed, and the cough to ariſe from this cauſe, I put him on a courſe of nitrous acid, from a dram to a dram and half a day, occaſionally diſcontinuing it for a few days, and directing oxyd of iron inſtead. In ſeven weeks he had no more cough; the pains were removed; the countenance was clearer; the fæces natural, and the pulſe between 70 and 80.—Query. Would nitrous acid be uſeful and ſafe in the qualms of pregnancy? Would it remove the violent effect of digitalis on the ſtomach and liver? What would be the effect of a ſolution of oxygenated muriate of potaſh in any of the above inſtances?

Caſe by Dr. Luke, *Phyſician at Falmouth.*

A gentleman in the naval line, between forty and fifty, had by the immoderate uſe of ſpirituous liquors, reduced himſelf to the laſt ſtage of diſeaſe conſequent to that unhappy practice. He had been compleatly jaundiced for ſeveral months, and at length, became ſo ſuddenly and generally dropſical, that he was after the firſt week, ſo unwieldly, as to be incapable of ſtirring from his bed.—Several remedies had been uſed by the Surgeon of his ſhip, and for the firſt few days after I ſaw him, the moſt active diuretics were employed, without effect, until an operation became neceſſary, to relieve him from the intolerable diſtention. The evacuation of five gallons of water, by means of the trochar from the abodomen, afforded ſome eaſe, but the extremities were ſtill enormouſly large, the pulſe 120 and weak; urine dark coloured, and not exceeding twelve ounces in twenty-four hours. In this ſtate, he was removed from his ſhip to lodgings, from an expectation that he could live but a few days

Directions were now reſumed, and ſuch medicines, as the moſt urgent ſymptoms demanded, but he was in two days as much diſtended as before the operation, ſo that it was propoſed, to repeat it the following day. The neceſſity of it however, was ſuperſeded by the burſting of the orifice of the firſt wound during the night, by which ſeveral gallons of water were ſoon diſcharged.

Repletion and evacuation occurred in this way, twice more in the ſpace of eight days, till at laſt, the operation of diuretics began to be apparent, and a ſufficient quantity was paſſed by urine, to prevent the uſual accumulation. He was now taking foxglove, and derived ſo much advantage from it, that his exiſtence was more tolerable, but the quantity of urine was not more than equal to the daily effuſion. Squills and calomel alkalis, æther, (Tickel's and common) were tried in their turn, during a month, without better ſucceſs.

For the laſt fortnight, the ſtomach and bowels had been much affected. A dark coffee-coloured fluid was diſcharged, both by ſtool and vomit, and theſe ſymptoms increaſed daily to ſuch a degree, that he could retain nothing in his ſtomach, and was often violently purged and griped. Effervefcing draughts, tinctura opii-pills, with calomel and rhubarb, and many other medicines were given, without effect. It was at this moment, through a forlorn hope, I determined to give mercurials in ſuch doſes as might, aided by friction, affect his mouth in a ſhort time. The accompliſhment of my purpoſe, was more ſpeedy than I expected. His mouth was ſore on the third day, and twelve hours had not elapſed, from the time of his firſt beginning to ſpit, before the vomiting and purgings were mitigated, the ſtomach was ſenſibly relieved, and to my great ſurpriſe and ſatisfaction, in the ſpace of a few days, retained food much better, than it had ever done ſince his illneſs.

Mercurials were continued, ſo as to keep up a gentle ſpitting for about three weeks, to which diuretics were joined as before.—But although the good effects above ſtated, immediately ſucceeded, no alteration had been effected in the ſize of the abdomen and extremities.—His ſtrength was nearly exhauſted, the pulſe exceſſively weak and quick, the lower extremities covered with petechiæ, heat and reſtleſſneſs increaſed; in ſhort, general ſymptoms of irritation demanded a ſuſpenſion of the mercurial courſe, in ſpite of any aid from opiates, corroborants, &c.

Under theſe circumſtances, I was induced from the character given of nitrous acid, to try its effect. A dram and half was diluted with a quart of water, to which were added, two table-ſpoonfuls of rum, and as much ſugar as he pleaſed. It proved rather grateful to him than otherwiſe, and was all taken with eaſe in the courſe of the firſt night. The ſame quantity was given daily, the next three days, when it was increaſed to two drams to two quarts. It ſtill agreed with him, and the rum was omitted. No other medicine was given at this time. He drank bottled cyder when thirſty. His diet conſiſted chiefly of gruel, with milk, to which he was very partial. In a few days he felt himſelf generally better, and was able to ſleep without anodynes. And at the end of a week, I had the pleaſure to obſerve, on my entering his room in the morning, ſeven or eight large rummer glaſſes full of wine diſcharged during the night, the colour ſtill dark, but more tranſparent. This was the firſt material change ſince the adoption of the acid, and the firſt that emitted the leaſt ray of hope. From this period, that is, the firſt week of taking this medicine, and ſix weeks from the operation of tapping, I found no occaſion to omit, or alter the preparation—He took it ſix weeks longer, daily, evacuating ſuch quantities of urine, as gradually reduced the ſwellings of the abdomen and extremities to their natural ſizes. A free ſpitting, and the ſame ſoreneſs of the mouth as was excited by mercurials, continued during the whole of this time, and the fœtor of the breath was unaltered. A bandage was now neceſſary, to ſupport the flaccid muſcles of the belly, and pills of cicuta were given on account of an induration, which would be evidently felt in the region of the liver. A liniment alſo was rubbed on the abdomen, twice a day, compoſed of camphorated oil and tinct. opii. With the exception of theſe remedies, none but the acid had been exhibited for ſix weeks paſt. His appetite and ſtrength had gradually improved—his ſtools were now natural and regular, and he ſat up ſeveral hours in the day. At the end of the next fortnight, he quitted his lodgings, and returned to his own houſe ſixty miles diſtant, ſtill taking the acid. About a month after his return, I heard from him, that he had left off the medicine, and was better than he had been for ſeveral years before.—I have this day, received a meſſage from him (ſix months having elapſed ſince I ſaw him) that he continues free from ſwelling, and in all reſpects better than he has been for years paſt, and requeſting that I will publiſh his caſe, not concealing either the name or cauſe of the complaint, if I think proper.

Obſervations on the foregoing Caſe.

Inſtances of recovery after tapping in dropſies, are I believe, by no means frequent. But in the preſent caſe, where the cauſe was of ſuch a nature that there could be no doubt about the exiſtence of diſeaſed liver, it is a fair preſumption to conclude, that the recovery is attributable to ſome extraordinary means.

The firſt important occurrence in the caſe, is the relief afforded to the ſtomach, by the action of mercury on the ſyſtem. It ſeems that the morbid ſecretion from the liver, ſtimulating the ſtomach and bowels to vomitings and purging, was corrected almoſt immediately as the mouth became affected. This event held out hopes, that perſeverance in the moderate uſe of this remedy, would produce further beneficial effects. But theſe expectations were fallacious. At the end of three weeks we found him in a ſtate that forbad the proceſs, and by the surgeon of his ſhip, as well as other medical gentlemen, who ſaw him, it was concluded, that his diſeaſe was beyond even the palliative powers of medicine. He was in this ſtate when the nitrous acid was firſt employed. Its effects are already detailed, and in offering them to the public, I pledge myſelf for a rigid adherence to facts.

I ſhall leave the reader to form his own inferences from this caſe, but muſt take the liberty to ſtate how far I conceive my patient indebted for his recovery, to the uſe of diluted nitrous acid. Although the previous exhibition and effects of mercury deprive the acid of an excluſive or ſpecific claim to the ſalutary change, no man will I conceive deny, that it proved a valuable reſource at the moment it was firſt uſed. I am ſtrongly impreſſed with its merits, and may be inclined to ſpeak of it with partiality; but taken even in a ſecondary point of view, as an auxillary to the work begun by mercurials, it claims the attention of practitioners in general, on ſimilar occaſions. If there exiſts another medicine capable of performing what it effected, viz : preſerving the ſame beneficial change as at firſt reſulted from mercurials, keeping up the ſalivation, while it improved the appetite and increaſed the flow of urine, ſo as gradually to remove all the dropſical ſymptoms, I was and am ignorant of it. Without it, I cannot ſay poſitively what the event might have been, but the proſpect was gloomy, and the iſſue would have been from all appearances fatal.

If theſe facts though in a ſolitary inſtance, can give any character to the nitrous acid, in the removal of dropſy, the ratio medendi offers reflections perfectly conſiſtent with them. Indurations, or ſome morbid ſtate of the liver, have been found productive of, or connected with, dropſy in general. If then, there be any truth in the repeated aſſertions of modern authors, that nitrous acid is ſpecifically uſeful in diſeaſes of the liver, the inference is obvious. From this conſideration I was induced to employ it; the event has juſtified the experiment.

Note from Mr. SCOTT, of Bombay.

I ſear that both you and the public are tired of me, but I think (excuſe my vanity if I be wrong) that I have ſtill ſomething new and uſeful to communicate. I have written another letter on the effects of the nitric acid bath on the human body, which I ſhall ſend you when opportunity occurs by ſea, but it is greatly too long to forward by this diſpatch which goes *(viâ Buſſora)* over the great deſert to Europe. If you think proper to publiſh the following paragraphs I ſhall be happy, as it may induce practitioners in Europe, to try the nitric acid bath from which I have experienced ſome very good effects. I write to nobody but yourſelf at preſent on this ſubject.

" Since I wrote you laſt, we have made a good many trials of the nitrous acid bath, which have confirmed the hope that I expreſſed ſome time ago of its being an uſeful remedy for ſome diſeaſes. By being applied to the ſurface of the body, I find that the acid, as I ſuppoſed, is abſorbed very plentifully by the ſkin; and that it performs the ſame effects in the ſyſtem which ariſe from its internal uſe. I have in many caſes, immerſed the whole ſurface of the body below the chin for half an hour daily, and with ſome people for twenty, or five and twenty days ſucceſſively. I make the bath ſo ſtrong with acid as to irritate the ſkin to a certain degree on which perhaps its abſorption depends. In about a week it brings on with many people a ptyaliſm and a ſoreneſs of the mouth and throat. It affected me, and it has others, in ſtill a ſhorter time. Theſe ſymptoms are accompanied or ſucceeded by an increaſed quickneſs of pulſe. I have ſeen it produce as violent a ſalivation as ever I ſaw from mercury, but I think this is not attended with the ſame kind of fœtor of the breath. By merely immerſing the legs in the acid bath for half an hour daily, I frequently ſee a ptyaliſm come on and continue while the bath is continued. Some people ſeem to be nearly inſenſible of theſe effects in whatever way the acid be applied.”

“ The acid bath if managed with any kind of prudence, is an extremely ſafe remedy. I have ſeen it do great good in chronic hepatitis under a variety of forms. I have found it of the greateſt ſervice in aſthma, and I have uſed it with advantage in fever. Like the acid internally it has an antiſyphilitic power. I have tried it in a number of caſes where mercury had entirely failed, and the reſult of my experience *ſeems* to be that I have cured two or three people under thoſe circumſtances. In ſeveral inſtances I have ſeen no kind of relief from it, but in a much greater number I have found it like mercury keep away the pains, &c. for a time, but they have returned again on leaving it off. It is well deſerving of the attention of practitioners, but my experience of it in this diſeaſe, is for too ſhort a time and to too ſmall an extent to enable me to ſpeak with the preciſion that I deſire. We have tried it too in caſes of recent ſyphilis for chancre and bubo: Under ſuch circumſtances it ſeems to ſucceed very well. It may be ſuppoſed that this ſucceſs ariſes from its internal action and that a relapſe is to be feared. This remains to be determined. Of all the means that I have ſeen of getting rid of thoſe ſymptoms, this is the leaſt injurious, the moſt agreeable, and ſometimes the ſpeedieſt. When it affects the mouth, I ſhould hope that the cure would be permanent: but allow me to ſay again, that I do not pretend to determine the point. Let the bath then in thoſe caſes be employed with doubt and with prudence, but let it not be rejected entirely; for if it ſhould be found to anſwer every intention, it will indeed be a bleſſing to all mankind. I ſhould think that public baths of this kind in London, would be of great benefit, and would in time, very well anſwer the end of any individual who ſhould inſtitute them. Such an eſtabliſhment would already find employment in this place."

"I conclude, by ſaying, that I am far from pretending to aſcertain the extent of the powers of the nitric bath as a remedy for diſeaſe, but I am certain that it is very conſiderable, and you may now *meo periculo* publiſh this as my opinion."

Several ſurgeons here, have promiſed to keep an account of the caſes in which they employ the bath, and which I hope to ſend you by the ſhips of the ſeaſon. I beg of you to try the nitric acid internally in dropſy. I your aſcites ariſes from the ſame cauſes with that of this country, I promiſe you great ſucceſs.

Cordially wiſhing you ſucceſs in your efforts for the progreſs of knowledge,

I remain, &c. &c.

Bombay. W. SCOTT.

Dr. Beddoes.

This note arrived in a letter, dated Nov. 2, 1798. I give it to the public without any comment except a reference to my *collection of teſtimonies on nitric acid* (Johnſon, 1799;) and the expreſſion of my belief, founded on ſome obſervation, that Dr. Scott and Dr. Luke are right in thinking that nitrous acid will be highly uſeful in dropſy.

EDITOR.

An Account of ſeveral Veins of Sulphate of Strontian or Strontites, found in the neighbourhood of BRISTOL, *with an analyſis of the different varieties.*

By WILLIAM CLAYFIELD.

The firſt ſpecimen of ſulphate of ſtrontian was ſhewn me by Mr. Tobin, about three years ſince. At that time it was generally believed to be merely a variety of ſulphate of barites. It had been found at Redland a ſhort time before, in a vein of conſiderable thickneſs.

The greater part of this vein has received a red tinge from the iron-ſtone on which it lies, and exhibits but ſlight traces of any regular chryſtallization. In ſome few ſituations however, it is entirely free from color, and appears to be compoſed of a confuſed maſs of bevilled tables, looſely adhering together. Its ſpecific gravity varies from 3. 51. to 3. 87. Walking along the beach at Auſt-Paſſage, in June 1797, I met with a ſimilar ſubſtance, and ſoon diſcovered ſeveral detached veins, in different parts of the cliff. The ſtrata in which theſe veins are found, are nearly horizontal, conſiſting of lime-ſtone, of different degrees of hardneſs, and argillaceous ſand-ſtone, intermixed with clay and gypſum. The whole cliff as well as the ſurrounding country, have evidently been produced by aqueous depoſition, ſince which period, the level of the water in the main channel having been conſiderably lowered, the Severn current has acquired ſufficient force to deepen the bed of the river, by plowing up the ſtrata, which had been previouſly formed. A ſecond depoſition of the ſoil towards the mouth of the river, by forming diagonal fiſſures in the cliff, has occaſioned an inequality of five or ſix feet in the level of the ſtrata.

Theſe fiſſures are moſtly filled up with veins of ſtrontian, from three to twelve inches in thickneſs, conſiſting of an aſſemblage of ſemi-tranſparent chryſtals, flanked up on each ſide by a thin layer, of a fibrous fracture; both of a delicately blue tinge. This laſt variety was obſerved about three months ſince, by Mr. Deriabin (inſpector of the Ruſſian mines) who was immediately ſtruck with its reſemblance to a ſubſtance of the ſame nature, found in Penſylvania.

The chryſtallization of the middle vein is either that of bevilled tables, or rhomboidal cubes, of nearly an inch in diameter, the tranſparency of the latter exceeds that of every other ſpecies; the ſpecific gravity of the cubes varied from 3. 88. to 3. 96, while that of the fibrous was about 3. 91.

Wiſhing to obtain ſome muriate of barites about nine months ſince, I reduced a portion of the ſpar to the ſtate of a ſulphure, and diſſolved the earth in marine acid; the great ſolubility of the ſalt, with its needle-formed chryſtals, ſoon indicated the preſence of ſtrontian.

Several trials which were then made with it, fully confirmed the reſult of the firſt experiment. Shortly after this, Dr. Beddoes informed me, of his having met with a paper of Klaproth's, containing an analyſis of the American ſulphate.

Since the firſt diſcovery of this rare production, Mr. Bright has furniſhed me with ſpecimens of another variety, from the neighbourhood of Ham-Green, where it is found breaking through the ſoil in ſuch large maſſes, that it has been made uſe of in mending the roads. The chryſtallization of the latter, like that of the Redland, conſiſts of bevilled tables; it does not however, partake either of its tinge or ſemi-tranſparency; its ſpecific gravity is between 3.60 and 3.68.

The preſent ſtate of the arts furniſhes continual inſtances of the refuſe of one manufacture forming the baſis of a ſecond. While this continues to take place, it is evident, that every new production muſt claim a full inveſtigation. The peculiar properties of this earth, renders it probable, that its affinities may ſhortly be made to furniſh us with thoſe productions from the raw materials of our own iſland, which we can now only obtain with conſiderable difficulty from other countries.

The following analyſis was undertaken at the ſolicitation of ſome chemical friends; more leiſure would doubtleſs have contributed to greater accuracy: What is now ſtated is the mean reſult of ſeveral experiments, the differences of which have rarely amounted to more than two or three grains. Conſidering the fibrous variety as the moſt deſerving attention, it was the firſt ſubjected to analyſis.

To find whether it contained any portion of water or other volatile material, 500 grains were expoſed to a red heat, under a muffle; the loſs amounting to no more than four grains, proves that the quantity of water, if any, muſt have been very trifling.

1. 200 grains of the powdered ſpar in its original ſtate were digeſted with a ſolution of carbonate of potaſh (obtained by deflagrating nitre and tartar); the powder, when dried in a red heat, weighed 163.5 grains.

2. A ſolution of this powder in diluted marine acid, extricated 47 grains of carbonic acid, leaving about 1 grain undiſſolved: this was afterwards taken up by the alternate appli-

cation of carbonate of potaſh and marine acid. From this it appears that the whole quantity of earth muſt have been very nearly 116.5 grains.

3. The ſolution No. 2 was then fully charged with cauſtic ammoniacal gas, which produced ſcarcely any traces of precipitation; the addition of carbonate of ammonia immediately threw down a precipitate, which dried as before, weighed nearly 160 grains; the difference in the weight of the precipitate, and that of the 162.5 grains taken up by the marine acid, aroſe from decanting the ſolution into different veſſels.

4. To detect any ſmall portion of barytes which might be contained in the precipitate, a quantity of marine acid was poured on it, ſufficient to diſſolve only a few grains of the earth. Had any barites been preſent, it would have been taken up in preference to the ſtrontian, from its ſuperior affinity for the acid; the ſolution, however, after digeſtion for ſeveral hours, ſtill chryſtallized in needles, and afforded a copious precipitate to baritic lime-water.

5. 164 grains of the precipitated carbonate of ſtrontian were thrown into marine acid, which

extricated as before, about 47 grains of carbonic acid, leaving nearly 1 grain undiſſolved; the addition of ſulphuric acid to the ſolution reproduced 200.8 grains of ſulphate of ſtrontian.

6. The ſolution of potaſh No. 1 was then taken, and the whole of the carbonic acid remaining in it diſengaged by an exceſs of marine acid; the addition of muriate of barytes afforded a precipitate, which after drying in a red heat, weighed nearly 249 grains; had the whole of the 200 grains been decompoſed, the ſolution would have furniſhed nearly 250 grains of ſulphate of barytes.

Klaproth and Dr. Withering having eſtimated the quantity of ſulphuric acid contained in artificial ſulphate of barytes at .33, and Fourcroy at .35 of the ſpecific gravity of 2.24, or that contained in ſulphate of potaſh; before the concentration of the acid contained in ſulphate of ſtrontian could be known, it was neceſſary to make the following experiments.

7. 218.5 grains of artificial ſulphate of barites, were decompoſed by digeſting with a ſolution of carbonate of potaſh, producing 190 grains of carbonate of barites, from which ma-

rine acid ſeparated 42 grains of carbonic acid, leaving nearly 148 grains of earth in ſolution; from this expt. it appears, that ſulphate of barites contains about 32.2. per cent of acid.

8. To find the concentration of this acid, 124 grains of ſulphuric acid of 1.843 ſpecific gravity, containing (according to Kirwan's table in the Iriſh tranſactions) 109.12 grains of ſtandard, or 97.42 grains of 2.24 were precipitated by baritic lime-water, producing 283.3 grains of ſulphate of barities, containing nearly 34.4. per cent of acid of 2.24 ſpecific gravity.

9. 92.2. grains of the ſame acid were precipitated by a ſolution of muriate of barites, the ſulphate of barites weighed nearly 212 grains, containing about 34.1. per cent of acid of 2. 24.

By taking the mean of theſe experiments, we may eſtimate the quantity of acid contained in ſulphate of barites at 33 per cent of the ſpecific gravity of 2.24.

According to this calculation, the 250 grains of ſulphate of barites, No. 6. would furniſh 82. 5. grains of acid of the above ſtrength.

10. To aſcertain the difference between native and artificial ſulphate of ſtrontian, 204.2.

grains of ſulphuric acid of 1.843 (containing 160.44. grains of 2.24. ſpecific gravity) were precipitated by ſtrontian lime-water, producing 360 grains of ſulphate of ſtrontian, containing about 44.5 per cent of acid. This accounts for the 200.8 of ſulphate being produced from 163 grains of carbonate of ſtrontian, No. 5 ; hence the proportion of acid in the artificial, will exceed that of the native nearly 2 per cent.

11. To determine whether the ſolution No. 2, contained any calcareous earth, a ſmall quantity of oxalic acid was added to it ; no precipitation however took place.

12. Pruſſiate of potaſh occaſioned a ſlight blue tinge.

The different varieties containing ſo nearly an equal proportion of earth and acid, the ſtatement of a ſingle analyſis will be ſufficient for the whole: Should there be any difference between them, it will probably be found in the Ham-Green variety's containing rather more acid : the quantity of ſulphate of barytes produced from it amounting to nearly 252 grains.

From the foregoing experiments it appears, that 200 grains of the fibrous variety contain

Strontian - - - -	116.5
Acid of 2.24 - - -	83.5
With a ſmall proportion of Iron	
	200.

In addition to the modes of diſtinguiſhing the two earths already noticed, we may ſtate the cryſtallization of the ſulphures.

A warm ſolution of the ſulphure of barytes depoſits on cooling, an aſſemblage of ſeveral very thin layers of inclined oval plates, terminating in points, and radiating from a center; while the ſulphure of ſtrontian runs into a baſe line, ſupporting a number of parallel perpendiculars gradually leſſening, ſo as to form the diagonal of a ſquare.

The ſtrontian earth in a ſtate of purity, frequently varies in its cryſtallization, ſometimes depoſiting ſolitary tables, and at others arranging them in regular lines.

Both barytes and ſtrontian combine with phoſphorus, and exhibit ſimilar appearances to the phoſphure of lime. Mixed with a few grains of oxygenated muriate of potaſh, and triturated in a mortar; an exploſion took place.

Note by the EDITOR.

Mr. Clayfield has lately been informed that another variety of ſulfate of ſtrontites is found near Sodbury. Mr. Deriabin has ſeen a blueiſh fibrous variety from a coal pit near Dumbarton. On a profeſſional jonrney to the North, I was ſtruck at Keſwick with a ſpecimen in Mr. Hutton's collection, labelled *ſtriated gypſum*. It is an exceedingly beautiful white ſulphate of ſtrontian from Alſton, as the label bears. From Mr. Hutton I have alſo received a blueiſh ſpecimen chrystallized in rhomboidal tables which I took for ſulpate of ſtrontian, but Mr. Clayfield finds it to be barytes. It comes from Cleter Moor, Cumberland. I have another from Newlands, Cumberland, having the exact appearance of the ſulphate of ſtrontian from Ham Green, which requires further examination. Many ſpecimens, ſuppoſed to be barytic, will doubtleſs, on examination, prove to be ſtrontitic. But the diſtinction will require nice inſpection, even from thoſe moſt verſed in the external characters of foſſils.

The following experiments were made at my requeſt by a friend.—12 grains of carbonate of barytes were given to a two months old rabbit. In half an hour not much affected—in an hour nearly dead. The barytes had acted violently as a cathartic, and had produced almoſt general paralyſis. In about two hours the rabbit died much convulſed. The ſtomach was greatly inflamed—the inner coat ſeparated from that below, lying in folds, and as if half macerated. 5 grains of barytes in a ſecond experiment, and 2 in a third, killed ſimilar rabbits, with the ſame effect on the ſtomach, only the fæces were not ſo much ſoftened. 40 grains of ſulphate of barytes had no effect. 12 grains of carbonate of ſtrontites, obtained from the Briſtol ſulphate, acted as a cathartic only; of the ſulphate itſelf 12 grains had no effect. This ſeems to ſhew that ſtrontites might be tried with little riſque as a medicine, if any analogy ſhould afford hope of benefit from it.

At my further requeſt, the following experiments were tried. 5 grains of carbonate of barytes, mixed with 5 of Cayenne pepper, were given to a ſimilar rabbit, which lived five hours and was not much affected till within an hour of its death; when it ſuffered as in the firſt experiment, and the ſtomach was ſimilarly injured. Conſidering that ſulphate of barytes has no action, by reaſon probably of its inſolubility, I thought it poſſible that innoxious abſorbents might engage the acid of the ſtomach, and prevent the effect of carbonate of barytes; in which caſe, under certain circumſtances, we ſhould be provided with an antidote for this deadly poiſon. The following experiments ſhew the conjecture to be erroneous: 5 grains of barytes, with 5 grains of prepared potaſh, were given to a rabbit. The animal ſeemed immediately affected, in leſs than an hour was ſcarce able to move, and in leſs than two hours died convulſed, with a ſtomach extremely injured as before. 5 grains of carbonate of barytes, with 20 of chalk, deſtroyed a rabbit in five hours. The animal died tranquilly. Carbonate of barytes 5 grains, with olive oil, killed a rabbit in leſs than two hours. The animal having remained all night unopened, the inner coat of the ſtomach was ſo much looſened as to be ſhaken out with its contents. Happening to try calcareous liver of ſulphur as an antidote, my friend obſerved the following extraordinary phænomena—

4 grains of carbonate of barytes, with a teaſpoonful of a ſolution of ſulphure of lime being given to a rabbit, two months old, the animal died immediately. The ſtomach being inſtantly examined, was found of a dark colour, and at its curvature (qu. *the great curvature ?)* converted into a yellow ſpungy ſubſtance, which looked as if burned by a cauſtic, and when removed left but a very thin membrane. The contents of the ſtomach near the darkened part, which ſeemed bounded by a whitiſh line, were alſo dark coloured. The *whole* inner coat was deſtroyed. As this rabbit had two days before been ſubjected to an experiment with ſulphate of barytes, it was neceſſary to aſcertain whether this had not had ſome ſhare in the unexpected effect. A tea ſpoonful therefore of the ſame ſolution of calcareous ſulphure was given to a full-grown rabbit. It had the ſame immediate

effect in deſtroying life; and the ſtate of the ſtomach was found to be the ſame. On the 26th of January 1799, a piece of ſolid ſulphure of potaſh about the ſize of a pea was given to a rabbit; and an hour afterwards another piece of the ſame ſize. A few minutes after the firſt doſe, the animal had convulſions of the ſkin, which were conſiderably increaſed by the ſecond. The whole head then began to ſwell, and in two hours the tongue was ſo enlarged as to be frequently bitten in attempts to maſticate. In two hours and a half the ſwelling was ſo large, and the breathing attended with ſuch difficulty and croup-like noiſe, that it ſeemed probable the animal had but few minutes to live. The operator, heartily ſick of theſe cruel experiments, and unwilling to ſacrifice more victims, gave the rabbit two tea ſpoonsful of olive oil, and was much pleaſed to find, that in ten minutes it appeared much relieved, and that the noiſe in reſpiration had ceaſed. The animal remained very ſtill, and after oil had been applied to the ſwellings, was left all night in a baſket of hay, in a warm ſituation.

Jan. 27th. The rabbit had left his baſket. A little warm milk was given him. In the evening, he eat his parſley and drank his water heartily. On the evening of the 28th, the power of the antidote being ſuppoſed to be aſcertained, the rabbit was killed, and a ſurgeon was deſired to examine the ſwellings. He found that they aroſe from an enlargement of the maxillary glands and of the lymphatics. The ſtomach was covered with a number of ſmall ſpecks, and ſeemed a little ſoftened.—It ſhould be obſerved, that this rabbit had been kept on dry food, which may account for the comparatively ſlow action of the ſulfure. An accident confirmed this. In the courſe of the experiment, having put his noſe into water, he received ſome on his tongue, which being ſwallowed, greatly increaſed his agonies. It happened ſhortly before the oil was given. The ſulfure had been carefully prepared. The experiment with the ſolution of calcareous ſulfure, was twice repeated. Each time the rabbit died in leſs than two minutes.—The ſtomach was black and yellow, ſpungy, and much coroded.

On the WHITENING *of* BONES.

The beſt method of whitening bones, has long been a deſideratum, upon which I have beſtowed no little attention.—The uſual cuſtom of expoſing them to the action of the ſun and air, will, under certain circumſtances, render their appearance extremely beautiful; but this, in large cities, where the atmoſphere is loaded with ſoot, is at beſt, very tedious, and frequently altogether impracticable. Waſhing them with muriatic acid, and ſoaking them in lime and alum-water, have each in their turn, been ſtrenuouſly recommended. I have tried each, and have in ſome inſtances, ſucceeded tolerably well; but I never ſaw effects produced, ſufficiently ſtriking, to induce the adoption of either plan, to the excluſion of the reſt. Bones are ſometimes whitened by being boiled for a length of time in water, ſaturated with kali. This plan is however, a bad one, for if the boiling be continued till the oil is *perfectly* extracted, they will be ſo ſoft, as to be unfit for demonſtration; and in the other caſe, they will retain their oily-brown appearance. This I learnt, after having ſacrificed two ſets of finger and wriſt-bones in experiments. It muſt be underſtood, that previous to any attempts to bleach them, all extraneous dirt, and putrid ſoft parts, had been compleatly waſhed away.

I am acquainted with only two methods of removing the integuments.

1ſt. By covering the limb with water in a convenient veſſel, and ſuffering it to remain in that ſituation for ſeveral months, when the muſcles, tendons, &c. will become a ſoft putrid maſs, from which the bones muſt be ſeparated by repeated waſhing.

2d. (Which I conſider as the leſs eligible of the two) conſiſts in expoſing the parts to the action of potaſh, rendered cauſtic by the addition of lime.—This is, however, very convenient, where diſpatch is neceſſary, as eight or ten hours, are in moſt caſes, ſufficient to effect the purpoſe. In my father's collection, is a carious tibia, prepared in this manner, which is by no means among the leaſt valuable ſpecimens in his muſeum.

In the ſpring of 1798, I was ſtruck with the wonderful effects produced by the bleaching liquor of the cotton and linen manufacturers: It immediately occurred to me, that its action upon bones was worthy of inveſtigation; and a favorable opportunity of trying it, ſoon after preſenting itſelf, I was ſo convinced of its efficacy, that I immediately fitted up an apparatus for the purpoſe.

I had at this time, a cranium in my poſſeſſion, lately prepared, perfectly clean, and inodorous, but ſo brown, that I had thrown it aſide, as unworthy a place in the muſeum: This, (together with the vertebræ of the neck) formed the ſubject of my firſt experiment: It was evident, that every good purpoſe might be anſwered by expoſing the bones to an oxygenated muriatic atmoſphere merely; and in order to accompliſh this, I adapted a cork with a ſtopcock driven through it, to the aperture made for the candle, at the bottom of a common lamp-glaſs. A Florence-flaſh which contained a quarter of an ounce of *black calx of manganeſe, mixed with half an ounce of muriatic acid*, ſerved for a retort, and a long-bent tube of glaſs fixed into its neck, enabled me to direct the product at pleaſure.

The ſkull had been immerſed for twelve hours, in a weak cauſtic ſolution of potaſh; but whether this is abſolutely neceſſary, I have not yet aſcertained.

The lamp which contained the head and ſeveral other bones, previouſly prepared in the ſame manner, was placed in a large trough of water. The ſtop-cock at the end, afforded me the means of filling it with water, by exhauſting the column of air above. Every thing being thus in readineſs, it remained only to diſlodge the water, by placing the flame of a ſpirit-lamp under the retort, and the extremity of the tube under the lamp-glaſs. In a few minutes, the yellow oxygenated muriatic acid gas, was evolved, and I perceived, that as the water ſunk, thoſe bones which were expoſed to its influence aſſumed the ſame appearance.

The operation was continued, till the glaſs was emptied; but I found it neceſſary to continue the proceſs ſlowly, in order to recruit the air which had been abſorbed by the preparation and water;—a circumſtance which was demonſtrated by the riſing of that fluid.

In ſix or eight hours I removed the glaſs, and expoſed the head, which was of a bright golden colour, to the open air, and the rays of the ſun; as it dried it became paler, and covered with ſmall ſhining chryſtals (which were afterwards waſhed off in rain water), and I had ſoon the pleaſure to find it ſo beautifully white, that it exceeded in appearance every thing of the kind which I before poſſeſſed. The dark caſe in which it has been ſome time encloſed, has rather diminiſhed its whiteneſs; a circumſtance which uſually takes place where the light is excluded: but it ſtill retains its beauty ſufficiently to demonſtrate the ſuperiority of this proceſs.

With old bones it does not ſucceed ſo well as with recent; but even theſe, in ſome caſes, were well bleached.

When the above experiments were made, I had not the leaſt idea that they would ever meet the eye of the public, conſequently ſo much attention was not beſtowed upon minutiæ, as would otherwiſe have been the caſe. If, however, you conſider this communication worth preſervation, it may poſſibly excite the attention of anatomiſts, to a branch of ſcience at preſent in its infancy.

I remain, ſir, &c.

Briſtol, Mar. 2, 1799. RICHARD SMITH.

Dr. BEDDOES.

Letter from Mr. ———, *Surgeon of* ——— *Hoſpital, on Gonorrhœa.*

Dear ſir,

I preſent you with the two following caſes of gonorrhœa, which were treated with the muriate of quickſilver, according to the method recommended by your correſpondent Mr. Addington, excepting the uſe of the glauber's ſalts, which at your requeſt were not exhibited. Theſe two caſes I purpoſely ſelected, as one of them was of very long ſtanding, the other of a recent date; and both the patients otherwiſe in good health. I am ſorry to ſay the reſult, as will appear on conſulting the caſes, was far from flattering; the medicine, which was faithfully prepared and adminiſtered, was taken for the few firſt doſes without producing any alarming ſymptoms; but as ſoon as I adminiſtered it in the form recommended, viz a grain and half of muriate of quickſilver, diſſolved in half an ounce of rectified ſpirit of wine, ſuch violent ſymptoms in both inſtances, and in Holt's caſe ſuch dangerous ones ſupervened, without any amendment in the diſeaſe taking place, that I ſhould not have held myſelf juſtified in perſiſting further in the uſe of the medicine.

Suppoſing cures to have been accompliſhed in almoſt every inſtance in which theſe large doſes of muriate are ſaid to have been given, as theſe cures could only have been accompliſhed by the violent local action which the medicine had excited on the mouth, fauces, throat, and ſtomach, ſuſpending and annihilating the diſeaſed action in the urethra; (for it appears to me to produce an action in the ſtomach, throat and fauces, ſufficiently violent to remove the diſtant diſeaſed action of the urethra,) it muſt be attended with ſo much hazard to the patient, as cannot warrant its general uſe; though it be granted that troubleſome caſes of gonorrhœa now and then preſent themſelves to our notice, in which ſtrong means are obliged to be had recourſe to. Had I not placed the greateſt confidence in your correſpondent's detail, I ſhould not have had the courage to have given ſo large a doſe of muriate of quickſilver, eſpecially when I reflected on two caſes which have occurred in this hoſpital during the war, in one of which dangerous, and in the other fatal conſequences, followed the exhibition of the muriate, though taken in ſmaller quantities than is now recommended in gonorrhœa.

The teſtimonies offered by Mr. A. are ſo ſtrong, that I hope the hints and failures which I now ſend, will not deter nor prevent other practitioners from entering on the uſe of this medicine, in whoſe hands I truſt it will be more fortunate.

I am, &c. —— ——

JOSEPH HOLT, a ſtrong athletic ſeaman, was admitted into the ——— at ——— on the 30th of January, 1799. He ſaid, that about fourteen months before, he had contracted a gonorrhœa, which, according to the deſcription of the ſymptoms he then ſuffered, could not have been very virulent: that a week after its appearance he made application to his ſurgeon, under whoſe charge and care he continued upwards of three months, at the expiration of which time, the diſeaſe inſtead of being cured had gained ground, in ſpite of various means which the ſurgeon had with great attention employed: that he then returned to his duty, with a diſcharge from the urethra, which always encreaſed on any exceſs being committed. About ten days before his admiſſion here, a new ſurgeon having been appointed to his ſhip, to him he made application: though he alſo exerted his endeavours, no amendment took place. When admitted into this hoſpital, I found that a conſtant and copious diſcharge of purulent matter flowed from the urethra, which was very tender, more particularly ſo about two inches from the orifice. The diſcharge was ſimilar in appearance and conſiſtence to that which is generally obſerved near the termination of gonorrhœa; he experienced now no ardor urinæ, no chordee, nor any inflammatory ſymptom; neither was there the ſmalleſt reaſon to ſuſpect either a diſeaſed bladder, proſtrate gland, or ſtrictured urethra: the patient's ſpirits were apparently oppreſſed from the duration of the diſeaſe, and he was very anxious and willing to enter on any plan that could be deviſed for his cure. January 31ſt ℞ hydrargyri muriati gr. i. ſp. vini rectif. ℥ i. miſce et adde ſyrup. ſimplic. ℥ ij. aq. menth. pip. ℥ i. m. pro. hauſt; which was given about noon. In about ten minutes a frothy ſalivary diſcharge (ſuch as we obſerve flow from the mouth of a dog when much exerciſed in warm weather) was excited, and continued two hours; quantity ſpat about a quart. Feb. 2d, a grain and quarter of hyd. muriat. mixed in the former manner was given; it produced the ſame frothy diſcharge as before, but it laſted only one hour. 4th, a grain and half of hydrarg.

muriat. compounded as before, was taken, which ſoon produced ſo much uneaſineſs in the ſtomach, as nearly cauſed vomiting, and obliged him to go to bed, which had not happened with the other doſes; ſpat but little. In about three hours he had ſeveral diſcharges from the bowels, after having endured much griping pain. 5th. This morning the patient declared that both the diſcharge and pain of the urethra were not "half as much" as they had been before coming to the Hoſpital. 6th. Repeated the draught as on the 4th, which produced efforts to vomit, with griping pain of the bowels; it did not, however, oblige him to lay down; ſpat little. 8th. The diſcharge from the urethra was not leſſened; the draught was repeated; ſpat about twenty minutes; it produced neither nauſea in the ſtomach, nor uneaſineſs in the bowels. 10th. Diſeaſe rather better; repeated the draught, which excited more ſpitting than uſual, and was followed in a few hours by ten diſcharges from the bowels, without pain. 12th. Gonorrhœa as at firſt; repeated the draught, which produced an inclination to vomit, which was allayed by walking out into the open air; ſpat about ten minutes. 14th. The following draught; ℞ hydrargyri muriat. gr. iſs. ſp. vini rectif. ℥ſs. m. was given, which on ſwallowing produced a violent pungent heat, followed immediately by ſpitting, which went off in about eight minutes; he was then compelled to ſeek his bed from the tormenting pain which came on in the fauces, ſtomach, bowels, and head; his bowels and head he deſcribed as on fire; ſevere vomitings and ſtrainings ſupervened; the ſtomach rejected every thing for the three following days. On viſiting him again about eight hours after taking the muriate, I found that theſe ſymptoms were much aggravated, with a full, quick pulſe, a dry furred tongue, a great confuſion in his manner and anſwers; the abdomen much tumefied, which was relieved by prodigious evacuations of flatus by the mouth and anus: theſe alarming ſymptoms in four days were ſubdued, but not till after much real anxiety had been experienced on my part, and ſevere pain and ſuffering on his. The diſcharge from the urethra, and other ſymptoms, which were certainly at one period better, were now as bad as before we entered on the uſe of muriate of quickſilver.

MICHAEL M$^{c.}$ CUE, a Marine, twenty-one years of age, was received into the ———— at ———, on the 31ſt. of January, 1799. having about ſix weeks before, contracted a gonorrhœa, for which he had taken a few powders and ſome mercurial pills, without producing any amendment. The day of his admiſſion hither, I found that he had a gonorrhœa which diſcharged freely, attended with ardor urinæ—a pouting of the lips of the urethra, with inflammation of the glans—a troubleſome chordee, and an almoſt conſtant inclination to void his urine.—Feb. 1. I gave him the following draught: ℞ hydrargyri muriati gr. i. ſp. vini rectif. ʒi. miſce et adde ſyrup. ſimplic ij. aqu. menth. pip. ʒi. m. pro. hauſt. In about fifteen minutes after taking it, a frothy ſalival diſcharge came on, and continued two hours; in which time, nearly a quart was diſcharged, producing no other effect.—3d. The gonorrhœa was not better—a grain and quarter of hydrarg: mur: was given in the ſame form, which produced uneaſineſs in the ſtomach and bowels. Spitting was brought on in about ten minutes, and remained an hour. Quantity ſpat, about a pint.—5th. The ardor urinæ, and chordee, were leſſened. The frequent inclination to urine, was gone off: a draught, containing a grain and half of hyd. muriat. mixed as before, was taken—In twelve minutes, a ſpitting was excited, and laſted an hour. The draught produced much pain in the ſtomach—general health was good.—7th. Repeated the draught as on 5th. It produced great inquietude, and immediate ſpitting, which ſubſided in about an hour. In the night he was much griped. The chordee returned with more violence, and the other ſymptoms were as bad as ever.—9th. The gonorrhœa was not better—the draught was repeated: ſpitting came on directly, and continued about forty minutes; quantity diſcharged, about a pint: no bad effects followed.—11th. Repeated the draught: ſpitting immediately took place, and went off again in about half an hour: quantity diſcharged very ſmall: gonorrhœa not mended.—13th. Gonorrhœa worſe. I gave him a grain and half of the muriate of quickſilver, diſſolved in half an ounce of rectified ſpirit of wine, which made him urge to vomit, and excited ſpitting the inſtant it was ſwallowed, which did not go off for three hours; during which, he ſpat about a pint and half.—So violent a pain came on over his eyes, as forced him to bed. Afterwards, he was much purged.—15th. Repeated the draught of the ſame form and ſtrength as the laſt—ſpat about a pint—vomited with great ſtraining for ſix hours; complaining of a burning pain in his mouth, throat, and ſtomach.—16. The gonorrhœa with all its attendant ſymptoms, being aggravated, it was not deemed prudent, or juſtifiable, to perſevere in the further uſe of the muriate of quickſilver.

Letter from MR. ADDINGTON.

WEST BROMWICH, *March* 5, 1799.

Dear Sir,

I beg of you to accept my beſt thanks for the printed ſheet which you was ſo good as to ſend me on Saturday, for your letter of the next day, and for the remainder of the reports on nitrous acid, received through the kindneſs of Mr. Watt, a few days ago. On the uſe of this acid in the treatment of ſyphilis, I have only now to add to my former teſtimony, that the diſappointments I then met with, have ſince prevented my relying upon it *ſolely* for a cure; but that I have, as far as I can judge, found it a very uſeful *auxiliary*.

With reſpect to the treatment of gonorrhœa by muriate of quickſilver, I can now inform you that the practice, ſince the date of my former communication, has been generally ſatisfactory; as I have no doubt would be ſhewn by a particular detail of the caſes that have ariſen, did the time in which it is neceſſary to ſend you this reply admit of its being made out. As this is impoſſible in the few hours allowed me, I muſt confine myſelf at preſent to a few remarks on the two caſes contained in the ſheet you have had the goodneſs to ſend me. Theſe remarks are ſo perfectly obvious on even a curſory view of the treatment in theſe inſtances, compared with that which was employed in the caſes I had laid before you, that they may be ſuppoſed to occur to every reader of both. Having however been the firſt to adopt, and as far as my communication goes, to recommend the practice referred to, it may perhaps be incumbent upon me to take ſome notice of its progreſs. In this view I am by no means sorry for the publication of the unſucceſsful caſes of Holt and Mc. Cue, tranſmitted by your correſpondent, as they may ſerve a valuable purpoſe in guarding and directing our future practice in the employment of a medicine of ſuch confeſſedly active powers. *Three* things are very obſervable in the ſtatement of your correſpondent, as material deviations from the plan I had followed: viz. firſt, the omiſſion of the glaubers ſalt after every doſe of the muriate. Secondly, the very ſhort intervals at which the latter was repeated; and thirdly, the great number of doſes given. The *firſt*, it ſeems, was by your direction, and I leave it to you to conſider what may have been its influence on the very different reſults both as to the failure of the expected good effects, and as to the violent and deleterious operation of the latter diſeaſe. If, as I have for the moſt part ſuppoſed, this part of the preſcription be directed leſs againſt the diſeaſe, than againſt the intervention of any pernicious effects of the muriate, the omiſſion of it may powerfully combine with the *ſecond* particular of deviation above mentioned. In the tranſcript of caſes which I ſent you, it will appear that a clear interval of two or three days was uſually allowed between the ſeveral doſes of the medicine, even the two or three firſt doſes; and when more than theſe were required to accompliſh the cure, the length of interval was extended, ſometimes to four, five, or ſix days, or more. There is an exception to this rule at caſe 10th, which was owing to ſome circumſtances which rendered expedition in the cure particularly deſirable to this patient, and I obſerved in him a remarkable inſenſibility, even to the *diſagreeable* ſenſations which are uſually attendant on the immediate operation of the medicine; and farther, it is to be obſerved that the ſalts were always given on the intervening days. The *third* particular which I have noticed as a deviation, conſiſts in the number of doſes given; for which it may be alledged, a neceſſity was created by the inefficacy of the medicine in the early exhibition of it. To this I have only to reply, that I have not found ſuch a number of doſes neceſſary in any inſtance hitherto. The diſeaſe has ſometimes been ſubdued by two of the draughts, moſt commonly by three or four, and ſix is the greateſt number given. In one of the inſtances, viz. Caſe I, I have ſtated that I believed the two, if not three laſt, to be wholly unneceſſary; and in the other, caſe VI, it is remarkable enough that *all* were ineffectual. But too much ſtreſs cannot be laid on the obſervation that the temporary advantage which your correſpondent ſtates to have been obtained even in theſe two caſes, followed upon the *early* doſes—one patient it ſeems declared after the *third* that both pain and diſcharge were leſſened more than "*one half*,"—and farther, that the violent and deleterious effects took place after the latter or laſt of the *eight* draughts which were adminiſtered in this quick ſucceſſion of every other day, without any intermediate precautionary meaſures. It is curious alſo to perceive, that when theſe ſevere and dangerous affections of the ſyſtem thus ſupervened, the diſeaſe, ſo far from being leſſened in proportion, actually regained its former ground and ſtrength; a fact, which at leaſt affords no ſupport to your correſpondent's ſuppoſition of the modus operandi of the medicine, and the hazard that muſt attend its general uſe. I have had occaſion to remark, that when the *ſtomach* and *bowels* have partaken moſt of its action, and vomiting and purging have been induced, its beneficial effect on the urethral diſeaſe has been leſs conſiderable than when its operation has been more confined to the fauces and mouth, with the ſalivary glands.—I am not inclined however, even if the time permitted, to enter into any argument on the ſubject; with the few facts yet obtained it would be premature. No one can entertain more ſerious apprehenſions than I did on the firſt view of the preſcription, nor enter on the exhibition of the medicine with more timidity or reluctance: with reſpect to the caſes already given, I can only aver, that they are faithfully related; and may now add, that in perhaps as many more which have ſince occurred, I have never once met with any of thoſe violent affections of the ſyſtem which took place in Mr. ———'s two patients. I have before ſaid that the ſucceſs though not uniform, has proved generally ſatisfactory.

I ſhall continue to record the practice with as much accuracy as I am capable of, and in the mean time ſhall be glad to obſerve the degree in which it ſucceeds in the hands of other practitioners who may be diſpoſed to ſubject it to the teſt of experiment under their own obſervation.

I remain, dear ſir, with true reſpect,

J. ADDINGTON,

Note by the EDITOR *on the uſe of mercury in febrile diſeaſees.* (See above, p. 377, Mr. HAMMICK on Dr. GEACH's practice).

There ſeems every reaſon to ſuppoſe that mercury has continued more or leſs in uſe in fevers and in pyrexiæ, ſince the riſe of the alchemical ſect, or before. In gout and rheumatiſm, and other complaints, it is much recommended in F: Hildanus, and Zac. Luſitanus. Boerhaave thought highly of it in ſmall-pox: Engliſh medical writers, a century ago, in inflammations. Its external and internal uſe in catarrhs, inflammations and proper fevers grew very common among Italian practitioners in the earlier half of the preſent century, as may be ſeen at large in Rotario *remedio alle catarrali moleſtie e a qualſivoglia inflammazione, Verona,* 1733, and in Morcali *Syſtema febrium malignarum Mutinæ* 1739. Some time afterwards we find bleeding and mercury employed in fevers in Italy full as freely as of late in the W. Indies, or in America, only that bark was ſometimes largely added to the mercury. Benvenuti *diſs. quâ epid. febres deſcribuntur, neconon et cort p. uſus Luccæ* 1754; deſcribes a fever, attacking particularly full and robuſt young people. Diſſection ſhewed either inflammation or mortification of the meninges of the brain, of the ſtomach, bowels or liver. At the onſet copious bleedings were ordered, and three or four times repeated. Then lenitive electuary with two ſcruples of *mercurius dulcis* was given, by which means Benvenuti reſtored many to their priſtine health. When this plan did not anſwer ſoon, a dram of bark was added to a ſcruple of the mercury, and of this powder a ſcruple taken every four hours for three days, when the fever commonly diſappeared. In deſperate caſes he gave three drams of bark with one dram of the mercury at once. In this way he ſays he cured many patients: his ſucceſs he aſcribes principally to the mercury; and declares he never obſerved any bad conſequence from the practice. A great deal of information may be found in the following learned diſſertation. J. J. Rambach *uſus mercurii in morbis inflammatoriis. Halae* 1794.

NOTE from DR. JENNER,

Reſpecting the preceeding FACTS *on* COW-POX.

I feel myſelf ſenſibly obliged by the candour with which you have communicated the letters of Mr. Cooke and Mr. Thornton, relative to the cow-pox and the ſmall-pox. I have neither leiſure nor inclination at the preſent moment to enter into an examination of their arguments, much leſs ſhall I attempt to refute the opinions of either of theſe gentlemen. I only think it neceſſary to inform the profeſſion, that I have a pamphlet juſt ready for the preſs, intended as a ſupplement to my former publication on the ſubject of cow-pox, in which I hope I have in a great meaſure ſucceeded in reconciling thoſe ſeeming contradictions which appear in many parts of this intereſting enquiry to have puzzled the minds of the public. I ſhall only at preſent obſerve, that nothing either of fact or argument appears in the communications of your correſpondents, which inclines me to alter the opinion I have formed of the nature of theſe diſorders.

On a peruſal of the letters of Mr. Cooke and Mr. Thornton, one remark obviouſly preſents itſelf, which indeed applies to every ſubject of human inveſtigation.—*That a candid and judicious public ſhould ſuſpend its deciſions on any point where its own benefit is concerned, until the whole of the facts and the conſequent rational deductions ſhall be laid before them.*—The ſame equitable tribunal perhaps, will not fail to diſcriminate between the man who ſeduloufly employs the greateſt part of his time in making experiments for the complete inveſtigation of a confeſſedly complex ſubject, and him who appears peremptorily to decide on the truth or falſhood of a theory, on the ſuppoſed authority of a few ſolitary inſtances, which after all may have been miſtated or miſunderſtood.

I remain, &c.

EDWARD JENNER.

26*th. Feb.*

To DR. BEDDOES.

Anſwers by Mr. JACOBS, *Attorney at Law,* BRISTOL, *to queries propoſed by the* EDITOR *reſpecting the* COW-POX.

1. *Are you acquainted with the cow-pox?*

A. The ſon of a farmer who held a large farm, I was, as ſoon as able, made to milk, when neceſſary from other avocations of ſervants (a large dairy being kept), and in that way heard of what was, and is called, the cow-pox.

2. *Have you had the cow-pox?*

A. Near fifty years ago, the dairy being affected with the cow-pox, I catched it in milking, and others who milked, were infected alſo. It ran through the dairy, and the diſorder might really be called dreadfully troubleſome. In the enſuing ſummer I had a ſlight infection, and it was partial only amongſt the cattle.

3. *How did it affect you?*

A. Sores like the ſmall pox; puſtules on every finger; the veins ſwoln, or much diſtended, and painful to the ſhoulder; and the fingers ſcarcely to be uſed, and when uſed, with much pain, as well otherwiſe as from the ſores. I do not recollect any ſwelling (beſides the diſtenſion of the veins) in the arms, but there was a ſwelling under the arms pretty conſiderable, but decreaſing as the cure proceeded.

4. *Had you general indiſpoſition or fever?*

A. I do not remember any particular indiſpoſition or fever; but herein my memory does not ſerve me very correctly.

5. *Had you any ſwellings, and where?*

Anſwered above.

6. *How long after you had the cow-pox were you affected with the ſmall-pox?*

A. I take it to be near ten years: and that I had been within that period, at various times, in great danger of taking the ſmall-pox from infection, to avoid which I was inoculated in London, and had a great burthen. The firſt inoculation not appearing on the eighth day to have taken effect, I was again inoculated, and from this had large numbers of puſtules on the extremities.

7. *Did you ſuppoſe yourſelf ſafe from the ſmall-pox after having had the cow-pox?*

A. By no means; at that time no idea, that I had ever heard of, prevailed, or had been taken up of ſuch an effect.

* * *

It is probably Mr. Jacobs, whoſe caſe is mentioned in a ſummary manner at the cloſe of an article by Dr. T. Bradley in No. 1. of the medical journal. The editor conceived it right to determine whether Mr. J's experience actually bore on the great queſtion. The anſwer to quere 3 ſeems pretty deciſive, eſpecially as Mr. Jacobs appears equally diſtinct and cautious in what he advances.

A LETTER to Dr. BEDDOES,

Containing obſervations on the uſe of Digitalis in Pulmonary Conſumption, with two Caſes in which it proved permanently ſucceſsful. By Nathan Drake, *M. D. Member of the Royal Medicinal Society of Edinburgh.*

Sir,

In a diſeaſe ſo generally fatal as phthiſis pulmonalis, and for which, though frequently ſought for, no certain remedy has hitherto been diſcovered, it ſeems the duty of every intelligent phyſician to purſue, if poſſible, an original plan, to aſcertain the effects of new medicines, or to reapply thoſe which, though poſſeſſing ſtrong powers, caprice, ignorance and apprehenſion have prematurely laid aſide. To your indefatigable perſeverance in the cultivation and application of chemical ſcience, we are indebted for a novel claſs of remedies, which bids fair to remove or alleviate ſome of the moſt diſtreſſing complaints incident to humanity. To the diſcaſe under conſideration you have particularly turned your attention, and ſought for aſſiſtance, not only from pneumatic chemiſtry, but from every quarter which held forth the proſpect of aid. It is therefore with peculiar pleaſure that, complying with your requeſt, I now communicate to you two caſes of phthisis, in which the digitalis purpurea of Linnæus has been employed with permanent ſucceſs. Though the exhibition of digitalis in conſumption be not abſolutly new, yet I truſt the mode in which I have adminiſtered it has a claim to that appellation, and the facts brought forward prove, what aſſuredly is of vaſt importance, that by the uſe of this medicine the pulſe may be lowered to forty ſtrokes in a minute, without any previous ſickneſs, and the depreſſion continued for weeks together with the happieſt conſequences.

The fate of this plant has been ſomewhat ſingular, for, from the ſixteenth century to nearly the preſent period, it has furniſhed a ſubject for applauſe to one practitioner, and for unqualified condemnation to another. Its earlieſt deſcribers, however, ſpeak of its ſucceſs in pectoral complaints, even Fuchſius (Hiſt. Plant. 893) to whom we are indebted for its name. and who firſt introduced it to the medical reader, has not forgotten its utility in theſe reſpects. Gerard and Parkinſon celebrated it as an expectorant, and Dr. Withering has given us a manuſcript note of a Mr. Saunders, found in a copy of Parkinſon's Herbal, which mentions conſumptions as infallibly cured by a decoction of fox-glove leaves in water. Salmon too, at the commencement of the preſent century, has noticed it in terms of laviſh commendation, and as he affirms his opinion to be the reſult of repeated experience, it may be worth while, notwithſtanding the daſh of empiriciſm which prevades all his writings, to tranſcribe the paſſage, "The ſpecific," ſays he, *Family diet*, *4th ed. p.* 144, "which tranſcends all the medicines here mentioned, and many others beſides, is the herb fox-glove. A weak decoction of the herb in water, or in wine, or in half water and half wine, may be drank as ordinary drink; and of the juice of the herb and flowers may be made a rob or ſyrup, with honey, which being taken, three ſpoons-full at a time, firſt in the morning faſting; ſecondly, at ten in the morning; thirdly, at four in the afternoon; and laſtly, at going to bed, will reſtore, where the patient is not paſt cure, beyond all expectation. It cures a phthiſis or ulcer of the lungs, when all other medicines have failed, and the ſick are eſteemed paſt cure: but as it is a very ſtrong medicament, and emetic withal, ſo it ought to be given with diſcretion, not to tranſcend the ſtrength of the patient, for then inſtead of doing good it may do hurt; and therefore the ſyrup ought to be taken at firſt in a leſſer doſe, and to be increaſed as you ſee cauſe. It opens the breaſt and lungs, frees them from phlegm, and cleanſes the ulcer and heals it when all other remedies act without effect. I have known it to do wonders, and ſpeak here from a long experience. Perſons in deep conſumptions, and given over by all phyſicians, have by the uſe of this herb been ſtrangely recovered, and ſo perfectly as to grow fat again."

That a medicine ſo powerfully inſtrumental in retarding the circulation, ſo liable to produce oppreſſive ſickneſs, together with pain and giddineſs in the head, ſhould have been frequently attended with alarming effects, and eſteemed by many even as an abſolute poiſon, is not to be wondered at, eſpecially when it is conſidered that among the poor, where it was at firſt chiefly uſed, its incautious exhibition would naturally lead to this concluſion. Even Ray, Boerhaave and Haller, mention its operation as generally deleterious; what however is truly extraordinary, none of the old writers, nor any of the moderns, I believe, previous to the year 1770, have mentioned its peculiar property as a diuretic, confining themſelves principally to its adminiſtration in epilepſy and ſcrofulous ulcerations. Its remarkable efficacy, however, in promoting abſorption has lately attracted due attention from the medical world, and this, together with its ſingular dominion over the motions of the heart and arteries induced Dr. Darwin and Sir George Baker to make ſome experiments with it in pulmonary conſumption, and in the third volume of the medical tranſactions, a caſe is related by the former, in which the digitalis was ſuppoſed to have operated a cure. In the Zoonomia, however, this celebrated phyſician has ſince informed us, that the diſeaſe terminated fatally. *(Zoonomia* II. 291*)* Here the medicine was given as in anaſarcous caſes, with a view to promote nauſea and conſequent abſorption; and indeed Sir George Baker, in his appendix to Dr. Darwin's remarks, obſerves, "if real benefit has at any time accrued to a conſumptive patient from the uſe of this medicine, may it not be conjectured, that it has been, at leaſt in part, effected by the action of vomiting?" The diſtreſſing ſickneſs, however, which this plant creates, and the fatigue to which the patient is conſequently ſubjected, led Sir George at the concluſion of his paper to wiſh, "that ſome mode or management could be contrived, by the means of which its power of promoting abſorption might be exerted, ſeparately from its virulent effects."

Such was the debility of the two patients whoſe caſes are ſubjoined to theſe remarks, that I was not willing to riſque the conſequences of ſickneſs, and became deſirous of introducing the digitalis in ſo guarded a manner as to preclude, if poſſible, all tendency toward an affection of the ſtomach, and yet at the ſame time of powerfully retarding the circulation, and of promoting abſorption from the lungs, and I the more readily conceived theſe to be attainable, as I had frequently, in the courſe of practice in anaſarca, obſerved a depreſſion of the pulſe and ſtrong diuretic effects from this herb, without any previous ſickneſs. That I ſhould be able not only to produce this retardation of the circulating fluids, but ſafely alſo to maintain it for weeks together, was I confeſs, an event more to be wiſhed for than expected.

As every phyſician is ſuppoſed maturely to weigh his motives for the adminiſtration of any medicine, and to form ſome theory of its operation at leaſt, and probable effects, it may not be unneceſſary in this place briefly to ſtate my views in preſcribing this plant in caſes ſo apparently deſperate.

It has been lately maintained by the moſt celebrated phyſiologiſts, among whom John Hunter ſtands foremoſt, that pus is a ſecreted fluid, the conſequence of certain diſeaſed motions of the extremities of the blood-veſſels; it has been likewiſe aſcertained, that hectic fever ariſes only from the matter of an open ulcer; that what is termed laudable pus, when ſecluded from the air, is neither capable of creating fever, nor, except by its gravity, can it irritate the parts on which it reſts. When pus, however, is expoſed to atmoſpheric air, it rapidly attracts oxygen, an acid of a peculiar kind is generated, and hectic fever, the effect of the abſorption of aerated matter, is produced. Now as an ulcer of the lungs is perpetually expoſed to a ſtream of air, and of courſe an ichorous poiſon continually forming by the union of oxygen with ſecreted matter, an important curative proceſs would ſeem to ariſe from promoting abſorption ſo rapidly from the ſurface of the diſeaſed parts, that the pus ſhall be taken up as ſoon as ſecreted, and conſequently its combination with oxygen prevented. If at the ſame time the medicine employed to promote abſorption, ſhould ſo powerfully retard the motion of the heart and circulating fluids, that the irritating and morbid action of the extremities of the blood-veſſels, and therefore ſecretion as its immediate effect, ſhould be conſiderably diminiſhed, if not altogether ſuſpended, another moſt ſalutary purpoſe would be accompliſhed. To the cautious and continued uſe of digitalis, Sir, I looked for theſe conſequences, though whether I ſhould be able to render them ſufficiently permanent to promote a cure, was neceſſarily a matter of great doubt. It was my wiſh alſo, as I have mentioned above, to effect theſe changes without any previous ſickneſs, concluding that ſhould I be able *gradually* to depreſs the circulation, nauſea, as a link in the chain of effects, might be excluded, and abſorption, together with a ſuppreſſion of morbid action and ſecretion, ſtill be the reſult.

It is a well-known fact, and probably ariſing from an indiſſoluble aſſociation between the ſtomach and the heart, that the pulſe generally ſinks in conſequence of nauſea; and as ſubſequent to the retardation of the action of the heart, abſorption frequently occurs, it has been ſuppoſed that nauſea, a diminution of arterial motion, and abſorption, are mutually and neceſſarily related to each other, and that were the firſt of theſe phænomena abſtracted, the latter, viz. abſorption, would not be produced. It has therefore been uſually attempted to promote pulmonary abſorption, through the medium of this affection of the ſtomach, whence the preſcription of emetics, of ſailing at ſea, and of ſwinging, to induce vertigo and ſickneſs; and the digitalis has been hitherto exhibited with this view. The caſes annexed, however, will prove that the circulation may be ſafely, powerfully, and perhaps more permanently retarded, independent of any affection of the ſtomach, and that abſorption as certainly follows a depreſſion thus procured, as when ſickneſs has uſhered it in.

The preparation of digitalis beſt adapted to my purpoſe, appeared to be the *ſaturated tincture*; and in the firſt caſe I commenced with but fifteen drops twice a day, in the ſecond twenty: Mr. Marris gradually encreaſed the doſe of tincture until he took one hundred drops: this quantity was firſt ordered on the

12th of July, when the pulſe beat but fifty ſtrokes in the minute, and was continued for nine days, when his pulſe dropped to forty. Beyond this depreſſion I thought it unſafe to proceed, and therefore immediately diminiſhed the doſe of tincture. During the interval between the 22d of June and the 17th of July, though the doſe of the digitalis had in this period gradually attained to its maximum, not the leaſt ſickneſs occurred, nor any one ſymptom that could lead to apprehenſion. On the 17th, however, a conſiderable intermiſſion of the pulſe took place, and continued for better than a fortnight, though without occaſioning to the patient the ſmalleſt uneaſy ſenſation. In Mr. Grimes's caſe the doſe of tincture was puſhed to ninety-ſix drops without inconvenience, and his pulſe fell to forty, yet no intermiſſion was felt; nauſea, however, and vomiting after his meals ſupervened the day after this quantity had been taken; theſe continued four or five days, and were only alleviated by the omiſſion of the digitalis.

Here therefore, in one inſtance, one hundred drops, in another ninety-ſix of the ſaturated tincture, were by gradually encreaſed doſes, ſafely introduced into the ſyſtem of very debilitated patients, before either ſickneſs or irregularity of the circulation appeared, and even then theſe ſymptoms proved of little moment, as the firſt was ſpeedily removed, and the ſecond produced no inconvenience. During this period all the ſymptoms of irritation and fever, cough, pain, and dyſpnœa daily grew better, and at length altogether retired. On the quantity and quality of the expectorated matter, the digitalis ſoon exerted a moſt remarkable effect, either promoting its abſorption, or diminiſhing its ſecretion, or perhaps both, in a rapid manner, whilſt at the ſame time it deprived it of its fœtor.

What, however, I conſider as of moſt importance in theſe caſes, and to which, perhaps, we are alone indebted for a cure, is the demonſtration of the poſſibility of retarding the circulation for weeks together, by the uſe of this medicine. In Mr. Marris's caſe the pulſe never roſe beyond fifty from July 12th to Auguſt 15th, nor in Mr. Grimes's from September 17th to October 8th! How greatly every ſalutary purpoſe, every curative intention, muſt have been forwarded by this permanent depreſſion of the circulating powers, muſt be obvious to every medical reader, nor will the limits of a letter allow me to expatiate farther on the ſubject; I ſhall only add, that the theory laid down, and the facts now given, will, if I miſtake not, mutually illuſtrate each other.

One circumſtance of diſparity in the two caſes as to the operation of the digitalis, ſhould be mentioned; it being neceſſary with Mr. Marris, after the pulſe had ſunk to forty-four, daily to perſiſt in the uſe of a doſe of the tincture, to maintain the depreſſion, and one day when the pulſe was below fifty, from a wiſh to aſcertain the reſult, the two doſes of the tincture were omitted, and the pulſe next morning beat 112, whereas in Mr. Grimes's caſe, though the digitalis was entirely omitted on the 24th of September, in conſequence of the nauſea, on the 2d of October the pulſe had not riſen beyond forty-eight.

The activity of this medicine is ſo great, that in caſes where much debility is preſent, the conſtant attendance of a perſon well apprized of its mode of operation and effects, ſhould be conſidered as abſolutely requiſite. Though Mr. Marris was at ſome diſtance from me, and I had not an opportunity of ſeeing him daily, yet was I free from any anxiety on that account, as the gentleman with whom he reſided, the Rev. John Hildyard of Monks-Eleigh, was not only well verſed in medical ſcience, but paid the moſt unwearied attention, both to the progreſs of the ſymptoms, and the exhibition and effects of the tincture. To his well-directed aſſiduity I think myſelf much indebted for the ſucceſs with which this attempt to cure conſumption through the medium of digitalis has been accompanied.

I may, I think, without heſitation affirm, that an early exhibition of the ſaturated tincture in conſumption, will in general prove ſucceſsful, and even when the diſeaſe is far advanced, provided the patient has but ſtrength ſufficient left to endure a gradual depreſſion of the circulation, a reſult equally fortunate may be expected. That this can be done, even in circumſtances of debility, to an extent adequate to effect a cure, and without either ſickneſs, languor, or loſs of appetite, the caſes now appended will ſatisfactorily atteſt. I have only to wiſh, ſir, they may contribute ſomewhat toward promoting the great and humane deſign in which you have ſo long and ſo laudably been engaged. I am, Sir, with great reſpect, &c.

NATHAN DRAKE.

Hadleigh, Suffolk, Feb. 21, 1799.

JAMES MARRIS, aged 16, complains of conſiderable difficulty of breathing upon motion, and of pain in the right ſide. He has a frequent ſhort cough, attended with a copious expectoration of pus, denſe, fœtid and occaſionally mixed with blood. Pulſe 120, and the morning and evening exacerbations, eſpecially the latter, ſtrongly marked. Great emaciation and proſtration of ſtrength. Tongue clean. Thirſt not conſiderable. Appetite not much impaired. Belly regular. Sleep interrupted, and he cannot lie with eaſe on his left ſide. Slight perſpiration toward morning. Skin hot and dry. Shivering fits every two or three days. Urine high-coloured.

He has ever been of a delicate conſtitution, and for two years or more liable to dyſpnœa and pain in his cheſt on uſing exerciſe, accompanied by a ſpitting of blood, and ſlight purulent expectoration. An hereditary tendency to phthisis exiſts in the family, his mother, aunt, and uncle having periſhed under that complaint. He is evidently likewiſe of the form and habit which phyſiologiſts conſider as predispoſing to tubercular conſumption. Having a few months ago removed from his native county of Lincolnſhire to a high ſituation in Suffolk, and ſhortly afterwards expoſed himſelf to cold whilſt fiſhing, he was attacked with a return of hæmoptyſis attended with ſtitches in his ſide, and fever. Theſe ſymptoms were mitigated under the care of a neighbouring ſurgeon, and he contiuued for a ſhort time tolerably well, but upon his difficulty of breathing, pain and fever recurring, though in a ſlight degree, he removed to Monks-Eleigh, beneath the care of a near relation. In about a week after this change of ſituation, his dyſpnœa and cough encreaſed, and he now began copiouſly to expectorate what appeared, and upon trial, with the vitriolic acid and cauſtic alkali, proved to be in a great proportion pus. This and the other ſymptoms ſtill augmenting, my aſſiſtance was required, and I ſaw him on June 22d, 1797, and had ſix ounces of blood taken from his arm, which on cooling proved conſiderable ſizy. I ordered a bliſter to be applied to his right ſide, and preſcribed ſome pills compoſed of myrrh and antimonial tartar, to be taken with neutral mixture three or four times a day.

June 26. Pulſe ſtill 120, with great heat, difficulty of breathing and cough. Expectoration, if any thing, encreaſed, and very fœtid. Pain rather mitigated. Great debility, ſo as to be incapable of walking acroſs the room without aſſiſtance. Little or no ſleep, and ſlight perſpirations towards morning.

R. Foliorum digitalis purpureæ in pulverem craſſum trit. unc. i.
Spiritus vini rectificati et
Aquæ puræ aa unc ii.
Digere leni calore, ſæpe agitans, per horas xxiv. et cola.

R. Kali pp ſcrup. i.
Succi limonis unc. ss.
Aquæ puræ drachm. vi.
Tincturæ digitalis purp. gutt. xv.
fiat hauſtus primo mane et horis duabus ante prandium ſumendus.

Ordered to take aſſes milk morning and evening, and allowed a little wine and animal food. June 28.—Pulſe 108; cough, breathing and pain rather eaſier. Debility appears to be encreaſing. Expectoration extremely copious and heavy. He is confined from weakneſs to his bed and couch, and his friends apprehend him not capable of ſurviving many days. No ſickneſs however from the digitalis. The tincture ordered to be encreaſed to twenty-five drops in each draught; more animal food allowed.—July 1ſt. Pain nearly gone; expectoration rather diminiſhed; cough not ſo frequent; breathing much freer; debility not encreaſed ſince the laſt viſit; ſkin cooler. Ordered thirty drops of the tincture in each draught.—July 3. Pulſe 80; breathing continues much eaſier; expectoration evidently diminiſhed, and the fœtor abating. He is capable of more exertion, and feels himſelf in every reſpect better, except that his appetite begins to fail. Ordered to take his draughts as on the 1ſt.—July 5. Pulſe 76; ſkin of the natural temperature, body open; no encreaſe of urine; expectoration greatly diminiſhed; breathing free, but ſtill unable to walk without aſſiſtance; no ſickneſs from the digitalis, but appetite much impaired.

R. Cinchonæ in pulverem tritæ unc. i.
Quaſſiæ ligni et.
Corticis aurantii aa drachm. ii.
Aquæ ferventis unc. xii.
Macera per horas tres et cola.

R. Infuſi colati unc. iss.
Acidi vitriolici diluti guttas x.
Tinct. digital. purp. gutt. xxxv.
Fiat hauſtus mane et meridie ſumendus.

July 8.—Pulſe 68. Appetite improved, being able to take much more animal food, and three glaſſes of wine per day; breathing eaſy; ſkin natural; expectoration rapidly diminiſhing. Ordered to take forty drops of the tincture in each draught twice a day.—July 12. Pulſe 50. Cough nearly gone; breathing perfectly eaſy; sleep sound; expectoration decreaſed more than two thirds; ordered fifty drops in each draught.—July 17. Pulſe 44; expectoration amounting but to three or four table-ſpoonful; no cough; ſtrength daily encreaſing, and he is able to walk about the houſe; appetite continues good; much intermiſſion in the pulſe; draughts twice a day as on the 12th. July 22.—Pulſe 40, and intermits every third ſtroke; no ſickneſs; expectoration amounts but to one table-ſpoonful, and when tried with the vitriolic acid evinces very little matter. No cough, no difficulty of breathing; appetite remains good, and his ſtrength continues to encreaſe; ordered to take only forty drops of the tincture in each draught.—July 28. Pulſe varying from 44 to 48, and ſtill intermitting; ſtrength ſo much reſtored as to be able to walk out for ſome time during the middle of the day. Other ſymptoms as on the 22d. Ordered but thirty drops in each draught.—Auguſt 4, Pulſe 48 and ſometimes 50; intermits every ſixth or ſeventh ſtroke; ſtrength almoſt reſtored, and he is rapidly gaining fleſh. Ordered but twenty-five drops of the tincture in each draught.—

Aug. 15. His pulſe has now never riſen beyond 50 for better than four weeks; his ſtrength is almoſt perfectly reſtored, and he can ride out for eight or ten miles with but little fatigue or uneaſineſs of any kind. His cough and difficulty of breathing are entirely removed, and his expectoration, which is now a mere trifle, is altogether free from pus. He feels no pain even from a deep inſpiration, and his countenance has a healthy appearance. Ordered to omit the digitalis, but continue the infuſion of cortex and quaſſia twice a day.—Auguſt 29, Pulſe 70 and free from intermiſſion; ſtrength perfectly reſtored; fleſh nearly regained, and in all reſpects indeed well.

CASE II. September 10, 1797. GEORGE GRIMES, aged 19, complains of very acute pain in his right ſide, which is increaſed by inſpiration; he has inceſſant cough, and great difficulty of breathing, accompanied with frequent expectoration, which is evidently purulent, and very fœtid. Pulſe 120, and hard. Complexion very florid; tongue foul; thirſt great; appetite much impaired; body regular; urine high-coloured, and depoſiting a copious ſediment; little or no reſt; has frequent ſhivering fits, and his health is rapidly declining. He has been liable for many months to ſlight pulmonary complaints, as cough, hectic fluſhings, and occaſional expectoration; and has been for ſome weeks taking cicuta under medical direction in town. It being thought adviſeable for him to try his native air, he arrived in Hadleigh about two months ago, and conceived himſelf for ſome time better from the change of ſituation. A few days ago, however, he was ſuddenly attacked with the ſymptoms above enumerated, after expoſure to wet and cold; and to alleviate which he has been twice bled and bliſtered by his ſurgeon, though without any material relief. It is neceſſary alſo to obſerve, that his father, mother, and ſiſter, all died of phthiſis pulmonalis in this place; the two latter I attended under this diſeaſe about five years ago.

In conſequence of the good effects I had experienced in the uſe of digitalis in Mr. Marris's caſe, I preſcribed as follows.

R. Kali pp. ſcrup. i.
Succi limonis q. ſ.
Lactis amygdalæ unc. i.
Tinct. digitalis purp. gutt. xx.
Fiat hauſtus mane et meridie ſumendus.

R. Mucilaginis ſem. cydonii mali et
Aquæ cinnam. unc iii. m. et ſumat cochleare largum urgente tuſſi.

Sept. 11th. Pulſe 112 and ſofter; cough very troubleſome; breathing rather eaſier; expectoration very copious; ſkin hot, and urine high-coloured; pain ſtill violent. Ordered twenty-five drops of tinctura digitalis in each draught. 12th. Pulſe 108; pain of the ſide abated; cough eaſier, and breathing more free; expectoration not quite ſo copious, and nearly free from fœtor; ſkin cooler. Body continues regular; tongue cleaner. Ordered thirty drops of tincture in each draught. 13th. Pulſe 94; expectoration diminiſhing; pain nearly gone; breathing eaſy; thirſt abated, and appetite returning; cough greatly better. Ordered thirty-five drops of tincture in each draught. 14th. Pulſe 80, ſoft and regular; expectoration reduced to half the quantity; pain entirely removed; ſlight perſpiration during the night; ordered forty drops of the tincture in each draught. 16th. Pulſe 64; expectoration continues rapidly decreaſing; cough nearly gone; no thirſt; tongue clean and natural; body regular; appetite good; perſpiration at night more profuſe. Draughts as on the 14th. 17th. Pulſe from 50 to 56. Other ſymptoms as on the preceding day. Ordered forty-five drops in each draught. 18th. Pulſe 50, and regular; expectoration amounts but to four table ſpoonsful; ſkin cool; cough perfectly removed; ſtrength manifeſtly increaſed. Perſpirations during the night continue. Draughts with digitalis as yeſterday:

R- Decocti cinchonæ unc. iſs.
Acidi vitriol. diluti gutt. xxv.
Fiat hauſtus omni nocte horâ ſomni ſumendus.

Sept. 19th. Pulſe 48. Other ſymptoms as yeſterday. Repetantur hauſtus. 21ſt. Pulſe 48; expectoration has no purulent appearance, and amounts but to three table ſpoonsful; perſpiration at night much abated; appetite much impaired.

R. Cinchonæ pulv. unc. i.
Quaſſiæ drachm. ii;
Aquæ ferventis unc. xii.
Macera et poſt horas iv cola.

R. Infuſi colati unc. iſs.
Tinct. cort. aurantii drachm ii.
—— digitalis purp. gutt. xxxxviij.
Fiat hauſtus mane et meridie ſumendus.

22d. Pulſe 40, and regular; ſome nauſea; little or no appetite; nightly perſpirations gone. Expectoration as on the 21ſt. Repetantur hauſtus. 24th. Pulſe 49, and regular; nauſea increaſed, and yeſterday and to-day he has brought up his food in a few minutes after ſwallowing it; expectoration to the quantity of about a table ſpoonful; body rather bound.

Omittantur hauſtus cum decocto cinchonæ, &c.
R. Lactis amygdalæ unc. i.
Sp. nucis moſchatæ drachm i.
Tinct. digitalis p. gutt. 50.
Fiat hauſtus primo mane ſumendus.

26th. Pulſe 40, and regular; nauſea continues, and his ſtomach is unable to retain food. Expectoration entirely free from pus; no motion for the laſt four days. Omittantur hauſtus cum tinctura digit.

R. Decocti pro enemate unc. xii.
Natri vitriolati, et
Olei olivæ aa unc iſs.
Fiat enema ſtatim injiciendum.
R. Emplaſtri ladani drachm vi.
Opii purificati, et
Olei macis per expreſſionem aa drachm i.
Fiat emplaſtrum regioni ventriculi applicandum.

28th. Pulſe 44, and regular; the ſtomach retains food, though the nauſea in a ſlight degree continues. Enema operated well.

R. Decocti cinchonæ drachm x.
Tinct. cinchonæ drachm i.
Acidi vitriol. dil. gutt. x.
Fiat hauſtus horis duabus ante prand. veſperique ſum.

29th. Pulſe 44, and regular; nauſea gone, and appetite perfectly reſtored. Pergat in uſu hauſtuum. October 2d. Pulſe 48, regular and ſoft; ſtrength greatly increaſed; breathing perfectly eaſy. About a ſpoonful of mucus evacuated daily, and without any mixture of pus; body open; appetite good, and in all reſpects free from complaint. 8th. Continued well until yeſterday, when remaining ſtationary in the garden for about two hours during the middle of the day, he caught cold. Pulſe 108; ſkin hot; face fluſhed; breathing quick and laborious.

R. Lactis amygdalæ unc. i.
Sp. nucis moſch. drachm i.
Tinct. digit. purp. gutt. xx.
Fiat hauſtus hac nocte horâ ſomni ſumendus, et cras primo mane repetendus.

9th. Pulſe 72; ſkin cooler; breathing eaſy; ſuffuſion on the face gone. Repetantur hauſtus cum tincturæ guttis xxv. ſingulis. 10th. Pulſe 60; ſkin cool; breathing eaſy; appetite good. Cont. hauſtus. 12th. Pulſe 48. In all reſpects well. Ordered to take only one draught with twenty-five drops of the tincture of digitalis every morning. 19th. Continued well, and with a pulſe varying from forty-eight to ſixty; and taking the draught every morning until to day, when he was attacked with a purging, and has had five looſe motions. In other reſpects well.

Omittatur hauſtus cum tinctura digit.
R. Miſturæ cretaceæ unc. vii.
Sp. Lavend. comp. unc. ſs.
Tinct. opii. gutt. xxv.
Fiat miſtura cujus ſumat cochlearia tria quater de die.

Ordered to omit vegetables and malt-liquor: Oct. 21ſt. Pulſe 56, and regular; diarrhœa abated. Repetatur miſtura. 27th. Pulſe 64; diarrhœa gone. In all reſpects well:

N. B. The diet, after his pulſe had ſunk to 50, was chiefly compoſed of animal food, and he drank three or four glaſſes of wine per day, with porter at his meals.

Letter from Dr. FOWLER, on the cure of CONSUMPTION.

I ſend you (to diſpoſe of as you may think proper) the reſult of moſt of the trials which I have hitherto made of the digitalis in caſes of pulmonary conſumption. As I began to give it more from a diſſatisfaction with the remedies uſually employed in this diſeaſe, than from any very ſanguine expectation of ſucceſs, I took no notes of many of the caſes in which I firſt uſed it. With reſpect to theſe I can therefore ſupply you with little more than the doſe of the medicine, and its more prominent effects.

Obſerving, however, that it in no caſe did harm; that on the contrary, it almoſt uniformly relieved the moſt diſtreſſing ſymptoms of the diſeaſe; and that in ſome it appeared even to have effected a cure, I began to collect as carefully as I could, all the material circumſtances of the caſes in which it had been given;

and the inſtances of its good effects, which I am now to lay before you, will I hope, induce you to avail yourſelf ot the many opportunities which muſt occur to you, for giving this ſubject a full and accurate inveſtigation.

As I had frequently ſeen large doſes of the digitalis given by others, and had myſelf ſtill more frequently given it in dropſical caſes, without ever oberving any of thoſe uncontrollable and dangerous effects,* which are ſaid to deter many from its uſe, my mind was perfectly at eaſe as to its probable effects in phthiſis, and the more ſo as its power of repreſſing arterial action, and inducing debility, from which we have moſt to apprehend in dropſy, was the very quality, from which properly directed, I hoped to derive moſt advantage here.

My attention was indeed firſt directed to it as a remedy likely to be uſeful in phthiſis, by its almoſt uniform effect of rendering the action of the arteries more ſlow than natural, at the ſame time that it appears to excite that of the abſorbents. It has long been known, that diſeaſed parts of the body may be removed by depriving them of *all* ſupply of blood from the arteries; and it is now known, that where this cannot with ſafety be attempted to ſo full an extent, on account of the intimate connection ſubſiſting between the part to be removed, and ſuch as we wiſh ſhould remain, that the ſame effect may be produced by diminiſhing to a certain degree the arterial ſupply of the part, at the ſame time that we leave the action of the abſorbents in full force. This is the purpoſe ſo ably effected by Mr. Hunter's ſcientific operation for the cure of popliteal aneuriſm: and I confeſs that I was not, and that I ſtill am not without hope, that ſomething analogous to this may be effected by the operation of digitalis on tubercles in the ſubſtance of the lungs. But my expectations of ſucceſs had a better foundation than reaſoning a priori.

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* I would by no means be underſtood to diſpute its power of producing deleterious and fatal effects, when given in quantities larger than what is warranted by (now ample) experience. This power I know it has; but it has it in common with many others of our moſt valuable remedies;—and no one ſurely would reject a good, becauſe from its abuſe it might poſſibly be productive of evil.

There was good ground to believe that Dr.

Darwin had cured one caſe[*] of phthiſis by a ſtrong decoction of digitalis.

Dr. Ferriar had certainly cured by an infuſion of this plant, four caſes of hæmoptoe, a diſeaſe nearly connected with conſumption; and in a note to page 18 of his 2d. vol. he ſays expreſſly, that he has "repeatedly ſtopped the progreſs of incipient conſumption by adminiſtering digitalis, when the patient was too much weakened by preceding diſeaſe to bear the uſual methods of leſſening the impetus of the circulating ſyſtem."

Dr. Withering's opinion of it (notwithſtanding his expreſſed wiſh that it may be further tried in this diſeaſe) was not, it is true, very encouraging; but it ſhould be recollected, that even with him it ſucceeded completely in one caſe, (No. cxx.) that it relieved another, (No. xl.) very far advanced, and that the remaining caſes in which it was given by him were loſt before recourſe was had to the digitalis.

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[*] I did not then know what the Dr. has ſince told us in his Zoonomia, vol. 2, p. 291; that this caſe (much relieved for a time) terminated fatally. He there relates another caſe certainly cured by it, but is in doubt whether this were not peripneumonia, or catarrh.

CASE I. The firſt caſe of conſumption, in which I had an opportunity of obſerving the effects of digitalis, was in a girl received into the Stafford Infirmary, under the care of my friend Dr. Edward Alexander, at the beginning of the year 1794. I have no notes of this caſe, but I perfectly recollect, that her ſymptoms impreſſed both of us with the opinion of its being incipient conſumption. Her cough was particularly troubleſome, and her pulſe very quick. The ſuggeſtion of the digitalis, I believe, came from me. She took it in powder, I think gr. i. two or three times a day. Her recovery was ſo rapid, and apparently complete, as not a little to pleaſe and ſurprize both of us. What afterwards became of her I had no opportunity of knowing.

The following caſes of out-patients, for whom I had preſcribed the digitalis at the Saliſbury infirmary, are by no means ſo full and detailed as I could wiſh them; but the unfavourable circumſtances under which medicines are adminiſtered to this very indigent claſs of patients, their expoſure to cauſes perpetually counteracting the effects of remedies, and renewing their complaints, together with the irregularity of their attendance, held out but little encouragement to note down the hiſtory of each individual's diſeaſe, at the time I began to give the digitalis. Finding, however, that all of them gave a favourable report of the effects of this remedy, that it uniformly freed them from the ſenſation of oppreſſive tightneſs about the cheſt, quieted their cough, rendered the pulſe more ſlow, and wherever hectic fever and profuſe night ſweats had taken place, put a ſtop to them; I endeavoured to aid my own recollection of them by the beſt accounts which I could collect from themſelves. The doſes of the remedies, and the periods during which they were uſed, are accurately tranſcribed from the books of the Infirmary.

CASE II. GEORGE MATTHEWS, aged 12, of a florid complexion, employed in the carpet manufactory at Wilton (a fertile ſource of diſeaſes of the cheſt), was admitted an out patient of the Saliſbury Infirmary, May 20, 1797. Symptoms, a violent, conſtant, and dry cough of ſome ſtanding; pain and exceſſive tightneſs about the cheſt; wheezing; ſhiverings, ſucceeded by fluſhings of heat every morning, and moſtly every morning after breakfaſt. Pulſe very frequent. Having kept him low, bliſtered him, and given him a ſaline mixture in the act of efferveſcence, till July 10, and with very little relief of his complaints, I ordered him to take unc. ſs. of the following decoction of digitalis every ſix hours till it ſhould produce ſickneſs; then to take it only once a day.—Folior. digitalis purpureæ recentium unc. ii. coque ex aq. puræ lb. i. ad colaturæ unc. viiſs, et adde tinct. cardamom. unc. ſs. On the 17th, I find it had not made him ſick: he was deſired ſtill to continue it, and a bliſter was applied to his ſide. On the ——, the digitalis was diſcontinued, and a decoct. cinchonæ cum acid. vitriol. dilut. was directed, together with opii. gr. i. omni nocte. Theſe laſt medicines were again repeated in November, after which I heard no more of him till January 1ſt, 1799, when he returned and informed me that he had continued perfectly well during the year he had been abſent, but that on catching a freſh cold, all his former ſymptoms had returned. I again gave him decoct. digital.* unc. ſs. bis quotidie. He did not come again to the hoſpital till the 29th, when I found he had taken only a pap ſpoonful twice a day; but ſaid the tightneſs acroſs his cheſt, pain, and hoarſeneſs, were much relieved. His cough was ſtill troubleſome. He was deſired to continue his medicine, but I have ſeen nothing of him, the diſtance and ſeverity of the weather having prevented his coming.

CASE III. Is of a man to whom emetics, ſalines, and bliſters, had afforded but little relief. I gave, while at the infirmary, unc. i. decoct. digital. His report of its effects at the next viſit were ſo favourable, that I gave him unc. ſs ſextis horis, with ſucci ſpiſſat. cicutæ gr. iii. omni nocte. Five days afterwards he made no complaint but of weakneſs. I therefore gave him the following medicine, in which I find him perſiſting at the time of his laſt viſit,

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* During the winter months, the apothecary, I find, has always made it with the dried leaves.

October 9, 1798.—Kali pp. drachm. iiſs, decoct. cinchonæ unc. viiſs, tinct. ejuſdem drachm. iv. m. ſumatur unc. i. ter quotidie adjiciendo ſucci limon. unc. ſs.

CASE IV. MARY WATERS, July 28th 1798. Of her I know no more than what I find in the hoſpital book. Sumat. decoct. digital. unc. ſs bis in die: admov. emplaſt. cantharid. lateri Aug. 11. decoct. digit. unc. i, decoct. cinchonæ unc. v. tinct. opii. gutt. xl. m. ſum. unc. ſs, ter in die-24, pergat. Though I ſaw no more of her, yet from my cuſtom of never ordering the bark while any difficulty of breathing continues, and from the circumſtance of this order having been repeated in this caſe, I think it may be ſafely concluded, that ſhe found herſelf well.

CASE V. ANN HUNT, aged 17, Aug. 11, 1798. On a ſuperficial examination, this firſt appeared to be one of thoſe caſes which Dr. Withering notices as ſo difficult to be diſtinguiſhed from chloroſis. The paleneſs of her lips contributed to miſlead me. Steel and bitters aggravated all her ſymptoms; but bliſters, with decoct. digit. unc. ſs bis in die, ſoon perfectly cured her.

CASE VI. ELIZABETH GEORGE, Sept. 8, 1798. After a bliſter and decoct. digital. unc. ſs. bis in die till 11th, decoct. of bark was combined with it. And on Nov. 3, after which I ſaw no more of her, I find her taking a mixture of myrrh and ſteel.

CASE VII. SAMUEL JAMES, aged 32, tall, thin, and pale, with a long neck, narrow cheſt, and high ſhoulders: for ſome years paſt a wool comber. While at Gibraltar, about nine years ago, had a cough with much white expectoration, for which he went to the hoſpital, and was cured in a month by emetics. His cough, with pain in his ſide, difficult reſpiration, and every ſymptom of confirmed phthiſis, again returned about a year and a half ſince, and continued increaſing till Oct. 6, 1798, when he applied to me. I ordered decoct. digit. unc. ſs. bis in die-23. he was directed to continue it every eighth hour. This he did regularly till Jan. 1ſt, 1799, when I found him in every reſpect relieved; but as he was not quite free from complaint, I ordered an infuſion of digitalis (unc. i. to aq. fervent. unc. viii.) with the addition of tinct. cinchonæ unc. ſs, pulv. ejuſdem drachm ii. On the 29th he made no complaint but of dyſpeptic ſymptoms, which were ſoon removed by rubig. ferri gr. viii. quaſſiæ pulv. gr. xii. bis terve in die. I have ſince ſeen him two or three times, and he continues, notwithſtanding the ſeaſon, perfectly well.

CASE VIII. RICHARD SMART, aged 14: Nov. 1799. This boy had every ſymptom of phthiſis in a ſtate of rapid progreſſion. A bliſter was applied to his cheſt on account of acute darting pains there, and unc. ſs of decoct. digital. was directed to be taken twice a day. On the 20th being much relieved, he was deſired to continue the ſame doſe. His pulſe at the next viſit, was found reduced to 68, and was rather irregular. His appetite, however, remained good, and he ſlept well. The digitalis was omitted, and decoct. cinchonæ, with the addition of kali and lemon juice in a ſtate of effervеſcence, ſubſtituted. On the 22d of

December, nearly well: continue. January 1ſt, 1799, much better: continue 22d: Apparently well; but I thought it right that he ſhould continue the effervescing bark mixture a little longer. I have ſeen no more of him.

CASE IX. JOSEPH YOUNG, a labourer, aged 50, admitted January 15, 1799: ſaid he did not conſider himſelf very ill till about chriſtmas, when on coughing, ſomething ſeemed to give way within him, and he ſpit up a great quantity of very fœtid matter. I gave him the decoct. digital. unc. ſs, ter in die, till the 19th, when having been made ſick by it, and being on the whole conſiderably better, he was directed to take it only once a day till the 26th. Being now much better, it was laid aſide altogether, and a decoct. of bark ſubſtituted. This he continued taking without any return of conſumptive ſymptoms on the 19th of February, when I laſt ſaw him, in good ſpirits.

I have ſtill ſeveral out-patients lately admitted, whom I am treating in the ſame manner with thoſe whoſe caſes I have detailed; and at preſent I ſee no reaſon to be diſſatisfied with the effects of my plan.

CASES *of* IN-PATIENTS.

THOMAS FITZGERALD, an Iriſhman, aged 32, admitted an in-patient o the Saliſbury Infirmary, July 13th, 1798. He ſaid he had been ill ten months, the three laſt of which he had paſſed in Haſler Hoſpital, from which he had been diſcharged as incurable. His appearance was ſo much that of a man who had not many days to live, that I refuſed to decide on his admiſſion, till requeſted by ſome of the governors to admit him. His complexion was very dark, his countenance haggard; his figure tall and extremely emaciated. His neck was very long, his ſhoulders high, and his cheſt narrow. His pulſe was ſmall, hard, and 130. He complained much of pains in his cheſt ſhooting through to his back. For this he had often been bliſtered, but without deriving any relief. He coughed inceſſantly, and his expectoration which was copious, had every appearance of pus. He had regularly two acceſſions of hectic fever every day. His legs had begun to ſwell but not to any conſiderable degree.

The ſymptom which engaged this man's attention moſt, was oppreſſive tightneſs acroſs his cheſt; and it deſerves to be remarked, that this was ſo much relieved in the courſe of a few hours after taking the firſt half ounce of the decoction of digitalis, that he told his nurſe he felt perfectly well, and thought he had no occaſion to trouble the charity any longer. He continued to take unc. ss. decoct. digit. quartis horis upwards of a week, without its either producing ſickneſs, or rendering his pulſe at all ſlower. Every other ſymptom had abated. An iſſue was directed to be formed in the part of his cheſt where moſt pain had been felt, and the digitalis was continued as before till the 28th, when he was ſo much better, that I gave him the decoct. of bark, and ordered the digitalis to be taken twice only in the day. On the 14th of Auguſt, his pulſe was perfectly natural, his cough and fever had left him, and he made no complaint. He remained a week or two longer with us—recovered his fleſh, and conſidered himſelf as perfectly well.

CATHARINE MUSSELWATE, with ſymptoms of phthiſis, admitted an in-patient, July 14. Took ipecac. pulv. ſcrup. i. On the 17th, began to take decoct. digital. unc. ss. ſextis horis: On the ſecond or third day was very ſick. The digitalis was now changed for the decoct. cinchonæ with kali and lemon juice in a ſtate of efferveſcence, as mentioned in former caſes. On the 31ſt. this was changed for Griffiths mixture of myrrh: ferrum vitriol. &c. On the 27th of Auguſt, I find I had again recourſe to decoct. of bark, but now with vitriol. acid, and on the 15th of September ſhe was diſmiſſed cured.

SARAH ALEXANDER, admitted July 30th. She took decoct. digit. bis die till Aug. 4th, when having been made rather ſick by it, and all the ſymptoms for which it was given having abated, I directed for her the bark efferveſcing mixture as above, and on the 7th, a bliſter on her cheſt. On the 19th Griffiths's mixture, and ſhortly afterwards ſhe was diſmiſſed cured.

THO. GIBBS and JOS. WHITE, both received cures by nearly ſimilar means.

MARG. NAYLOR, aged 36, a ſervant, about 12 years ago, after the uſual ſymptoms of hæmoptoe, ſpit a very large quantity of blood. About a year afterwards, on coughing, ſhe bled profuſely from the noſe. This very frequently returned in exceſs; and as ſhe did not menſtruate, the late Dr. Jacob ordered her to be bled, which ſhe was every five or ſix months, during three years. On neglecting to be bled, cold ſhiverings, followed by profuſe ſweats, attacked her; and ſhe has ever ſince had cough, pain in ſome part or other of her cheſt, a ſenſe of fulneſs and oppreſſion there, and a quick pulſe. I ſaw her for the firſt time in October laſt. During the ſix weeks previous to this, ſhe had been more than ever tormented by acute pains darting through her cheſt and between her ſhoulders, attended with ſhortneſs of breath and conſtant fever. For a conſiderable time ſhe had ſcarcely been able to lie down, and had had no ſleep. Her pulſe at this time beat 140 in a minute. I gave her Dr. Darwin's decoct. digit. which produced exceſſive ſickneſs, and after the firſt day relieved entirely her pains and difficulty of reſpiration. I now gave her opium in ſmall doſes for about a week, and ſhe became freer from complaint of every kind than ſhe had been for years before. She has lately had another attack of the ſame kind, but aggravated by an evident effuſion of water externally, and every ſymptom of its affuſion into the ſubſtance of the lungs. Digitalis again relieved all her complaints, and I ſaw her to-day in what ſhe conſiders as very tolerable health.

Sometime about April laſt, I was conſulted by the butler of a family in which I was attending. He appeared to be about 40, had lived freely and irregularly in every reſpect; he had every ſymptom of an incipient phthiſis. As I knew I ſhould ſeldom have an opportunity of ſeeing him, I did not venture to give him digitalis, but deſired him to live as much as poſſible on milk, and to uſe as common drink a ſaline mixture in the ſtate of efferveſcence. I ſaw no more of him till the 18th of

September, when his cough and all other ſymptoms had very much increaſed, and what he ſpat was yellow, and of a very diſagreeable taſte. I now gave him (as he was to be near me) unc. ſs. of a decoction of digitalis, made by boiling unc. i. of the freſh leaves in unc. viii. of water, till reduced to unc. v. and deſired him to repeat the doſe every eight hours till ſick. As it had not produced this affection the 21ſt, I deſired unc. ſs of a rather ſtronger decoction to be taken every four hours; after the fifth doſe he vomited, and continued to do ſo (though the medicine was diſcontinued), for three days. All his ſymptoms were not gradually, but almoſt inſtantly relieved. Opium in ſmall doſes, as mentioned by you in the *medical facts and obſ.* put a ſtop to his ſickneſs, and he continued to take it during ſeveral weeks, on account of a return of cough. Some months have now elapſed ſince I ſaw him; and a companion of his informs me, that he has become the father of a child, is married, and ſo far as he knows, in tolerable health.

A gentleman, who for ſome years had been tormented with pain in his cheſt, and at different times had ſpit large quantities of matter, applied to me laſt ſummer with every appearance of confirmed phthiſis, combined with dropſical ſymptoms, particularly thoſe of hydthorax. As I was not often to have an opportunity of ſeeing him, I gave him decoct. digit. unc. ſs ter quotidie, till ſickneſs was induced. It relieved all his ſymptoms in a few days, and in a month he was capable of conducting a buſineſs requiring conſiderable activity, and conſidered himſelf better than he had been ſince his firſt attack. He died lately, and ſo far as I can learn from his ſon, in conſequence of a ſudden effuſion of water within the cheſt. I have every reaſon to believe that he was a very intemperate man, and that he died a victim to his own imprudence.

The following caſe I think you will conſider with me as particularly valuable. I regret that I have no notes of it, and muſt relate it wholly from memory. A ſervant girl aged 20, tall, fair, and of that form which at firſt ſight leads one to apprehend conſumption, complained to me laſt May, of great heat in the palms of her hands, irregular ſhiverings, cough, pain in her cheſt, and oppreſſed breathing. Her pulſe was ſmall, hard, and very frequent. Theſe ſymptoms had continued ſome months, but not always in the ſame degree. The bark, which ſhe had taken from an idea that her complaints aroſe from weakneſs, ſhe told me, had increaſed her heat, cough, and tightneſs acroſs her cheſt in a very alarming manner. I gave her the digitalis in the following manner:— Folior. digital. recen. unc. ſs, coque ex. aq. puræ unc. v, ad colaturæ iiſs, et adde tinct. cardamoni unc. ſs, ſumatur unc. ſs, bis quotidie. It occaſioned a very ſlight degree of ſickneſs, and in the courſe of a fortnight freed her from all her complaints except of weakneſs, which was in a ſhort time relieved by decoction of bark in a ſaline draught. She has ever ſince continued perfectly well. Several of her family had died conſumptive.

I was deſired in July laſt to ſee the wife of a publican, aged 40; ſhe had been a healthy woman, and was the mother of ſeveral children. She complained of acute pains in different parts of her cheſt, and a ſenſe of great tightneſs there; coughed a great deal, and ſpit what had every appearance of matter tinged with blood. Her pulſe was very frequent, and ſhe had regular paroxyſms of a hectic fever, with profuſe ſweats towards morning: her legs were œdematous, and ſhe made but little water, of a high color. Theſe ſymptoms had been gradually increaſing during two months, and were attributed to a cold caught while lying-in. I gave her Dr. Darwin's decoction of digitalis unc. ſs quartis horis, with opii gr. i· ſcillæ gr. iſs, every night. She was ſoon freed from all her bad ſymptoms, and Griffiths's mixture made effervescent, ſoon reſtored her ſtrength and ſpirits, and ſhe is now, and has been many months, in perfect health. Call this caſe what you pleaſe, I am aware that not only it, but ſeveral of the others require comments, but I have not at preſent time to make them, nor even to read over what I have now written.

R. FOWLER.

TO DR. BEDDOES.

Addition by the EDITOR.

I had often given digitalis in ſmall doſes; and perhaps there are few phyſicians in the kingdom who have not trifled with this medicine in conſumption. In conſequence of a letter from Dr. R. Fowler, received about three months ago, I returned to its uſe, under a conviction that neither my previous want of ſucceſs, nor that of others, formed any objection to its efficacy, when adminiſtered with greater perſeverance, and in larger doſes.

A good many of the caſes in which I have hitherto tried it in effective doſes, were *lost caſes*, that is, the patients were in the latter part of the laſt ſtage of conſumption. I have not yet been fortunate enough to reſcue any patient in ſuch a ſituation. But in moſt inſtances there was a great alleviation of ſymptoms; in none did life appear to be ſhortened by the medicine; in ſome, as far analogy enables me to judge, it was greatly protracted.

What I obſerved principally in ſuch deſperate caſes was as follows. I do not conceive a minute journal of each can anſwer any uſeful purpoſe. Mr. ———, with black hair and dark eyes, had, as I collected from his account, contracted pulmonary conſumption from expoſure to cold. On his arrival at Clifton, a violent hæmorrhage from the lungs came on, and was ſucceeded by ſeveral others in the courſe of a few days. Ceruſſa acetata was largely adminiſtered; and during this time, the cough and hectic fever were as uſual ſuſpended. About the time of the laſt attack of hæmorrhage, I received Dr. Fowler's notice of his ſucceſs with digitalis; and I preſcribed the decoction of the recent leaves in doſes of half an ounce, ſometimes twice, and ſometimes four times in the twenty-four hours. The patient ſurvived five weeks, and ſeemed to die of inanition. He never had any return of rigor, heat, or night ſweat; the cough, which is commonly aggravated on the ceſſation of hæmorrhage, was moderate, and expectoration eaſy. He was not made ſick; his pulſe was never brought below 70.

Mr. ———, juſt able to travel, and obliged to take to his bed ſhortly after his arrival; with light hair and eyes, took the decoction in doſes ſometimes of two, ſometimes of four drams. His night-ſweats were leſs, and his breathing, as he thought, eaſier. I was afraid, as in the former caſe, of inducing ſickneſs. Neither patient had diarrhœa. Three other perſons, in nearly the ſame ſtage of the diſeaſe, took the decoction without apparent alteration. But I employed it with great caution.

A lady with light hair and eyes, in the beginning of the laſt ſtage of conſumption, that is, with cough, hectic fever, purulent expectoration, her feet juſt beginning to ſwell at night, and ſtools occaſionally looſe and frequent, took half an ounce of decoction of digitalis every ſix hours; the ſeventh doſe excited violent bilious vomiting, with no preceding diminution of the frequency of the pulſe. Her expectoration had been very copious, and a large quantity of frothy mucus was always mixed with the purulent matter. The morning after the ſickneſs, there was not in the expectoration a quantity of frothy mucus equal in ſize to a pin's head. The wheezing, which had been very troubleſome, ceaſed; and the ſwelling of the feet abated. Before the ſickneſs went off, the pulſe fell from 112 to 70, and became irregular. For a month ſhe took the decoction in doſes of one, two, or four drachms, twice or thrice in 24 hours. The pulſe never aroſe to 90 but once. The expectoration diminiſhed at firſt, but afterwards increaſed while the pulſe was about 70 and occaſional nauſea was felt. Twice vomitings of bile occurred, and after each there was a diminution of the expectoration. The night-ſweats returned no more. The rigors and heats, which every day for many months had come on in the morning or about noon, were never once felt. The cough was much diminiſhed. The general ſtate of the patient was oppreſſive languor; and I often queſtioned within myſelf whether ſhe ſuffered greatly leſs from this languor than from the violent cough and ſtrong hectic fever, which it had ſuperſeded. This patient had, except within a few hours of vomiting, an increaſed appetite under the digitalis. She expired placidly, after complaining greatly for a few days before of ſhortneſs of breath. She ſurvived the firſt adminiſtration of digitalis near two months. During the latter three weeks ſhe took tincture of digitalis, as recommended by Dr. Drake. But ſhe could never take 30 drops without nauſea, and 20 twice a day kept the pulſe at about 70, though not without great languor. Under the uſe of digitalis the patient ſlept much more than before, without any increaſe of opium. The ſtate of the bowels ſeemed little altered by this medicine. The chalk mixture was ſometimes neceſſary.

A lady with light hair and eyes had expectoration of purulent matter with mucus; pulſe 108—120. When I ſaw her there was no ſymptom of hectic fever, except pretty frequent night-ſweats; but ſhe had great pain of the bowels, with ſix, ſeven, or more lax ſtools in the 24 hours. She had long taken from 200 to 300 drops of laudanum daily. Extract of logwood largely given, with a diet of rice milk and hartſhorn jelly without admixture, permanently reduced the ſtools to one, two, or at moſt three in a day, and much abated the pain. Early in Jan. 1799, ſhe began to take of decoction of digitalis half an ounce every ſix hours, the laudanum being continued as uſual. The ſeventh doſe produced bilious vomiting, which was followed by a reduction of the pulſe to 76, with conſiderable irregularity. After two days ſuſpenſion, the digitalis was continued in doſes ſo regulated as not to produce vomiting. Loſs of appetite, with great languor, and almoſt conſtant ſleep followed, and continued a fortnight, but with no further reduction of the pulſe, or of the expectoration. She was ſometimes nauſeated, and then always complained of pain in the bowels; but no increaſe of diarrhœa took place. The medicine being diſcontinued, the patient emerged from her torpor into a ſtate of great eaſe and vivacity, attended with good appetite. Suppoſing I had given the medicine too freely, after a week's interval I ordered it again in a doſe of one dram at the interval of eight hours; but nauſea was twice diſtinctly produced by this cautious attempt to renew the trial of it.

After an interval of about a fortnight, an account of Dr. Drake's ſucceſs with the tincture reached the ears of the patient's friends, who were determined that no chance of recovery ſhould be left untried. A letter from Dr. Drake put me in poſſeſſion of his method, according to which the medicine ſeems ingeniouſly oppoſed to the two exacerbations of hectic fever.

About this time the pulſe had increaſed to above 100. The expectoration was as before the uſe of the decoction of digitalis. On Saturday the 16th of February, ten drops of the tincture were preſcribed; and the medicine was raiſed by cautious ſteps to 34 drops twice a day, the laudanum being continued as before. The pulſe was reduced to between 85 and 90. The ſecond doſe of 34 drops produced bilious vomiting, though no nauſea (and hardly any languor) had preceded its adminiſtration. There was much more than uſual ſleep all this time, and the appetite had been excellent. There had been no night ſweats. The ſickneſs laſted two days, and the tincture was recommenced in the doſe of eight drops twice a day. For as there was no rigor, heat or other hectic ſymptom, there was no reaſon for adminiſtering the medicine at particular times of the day. It ſeemed to be adviſeable to give as much as poſſible of the tincture without occaſioning any diſturbance of the ſtomach. The doſe being gradually raiſed to 20 drops thrice a day, bilious vomiting was produced.

To ſhew the relation between the doſes of the tincture and ſtate of the pulſe, I ſhall make a ſhort extract from the journal of the caſe. I muſt premiſe, that though it now was propoſed to give the digitalis thrice a day, yet if any conſiderable nauſea was felt, only two doſes were directed, or but one. I ſay *conſiderable nauſea*, for not one of the following days paſſed without ſome nauſea; and we had before proved that with fewer drops the pulſe roſe to near 100.

Sat. March 9th. 17 drops, thrice in 24 hours.

Sunday 10th. the ſame. Expectoration leſſened ſince Thurſday, the day of bilious vomiting.

Monday 11th. 17 drops morning—20 afternoon—very ſick after. Pulſe 68 morning—100 afternoon—expectoration increaſed.

Tueſday 12*th*. 15 drops morning—ditto at 4 p. m. Pulſe 86 morning—3 p. m. 68—midnight 80.

Wedneſday 13*th*. 15 drops 5 a. m.—16 2 p. m.—16 midnight. Pulſe 70 morning—2 p. m. 72—night 80.

Thurſday 14*th*. 17 drops at 11 a. m.—ditto at 4 p. m.—ditto at 12. Pulſe 72 morning—100 at night.

Friday 15*th*. 17 drops morning—ditto at 7 p. m.—ditto at 12.* Pulſe 66 morning—night 78.

Saturday 16*th*. 17 drops morning—ditto at 7, p. m. and at 12. Pulſe 70 morning—80 4 p. m.—80 night.

Sunday 17*th*. 19 drops morning—18 7 p. m.—ditto night. Pulſe morning 80—ditto at night.

Monday 18*th*. 18 at 10 a. m.—18 7 p. m.—ditto at night. Pulſe 86 morning—72 3 p. m.—98 at 12.

Tueſday 19*th*. 18 drops thrice. Pulſe morning 80—at 2 p. m. 98.

All theſe days there was more or leſs nauſea, though the appetite was good, and animal food, with one or two glaſſes of wine, taken twice. To day (19th) it has been without intermiſſion, and was ſtrong when the pulſe was 98. The expectoration, after many variations, is nearly what it was before the tincture was begun: the cough has varied, but has been much better upon the whole than without the digitalis. The bowels have continued as after the relief from the logwood. Strength upon the whole diminiſhed.

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* I directed theſe doſes ſo cloſe together, becauſe the pulſe was 100 in the preceding night between 12 and 2.

Rev. J. G. with black hair and dark eyes, had hæmoptoe about 18 months ago, which was ſucceeded by purulent expectoration and hectic fever. I ſaw him in May 1798, and adviſed a continuance of travelling, from which he had before experienced relief. In the courſe of the ſummer it at times nearly ſuſpended all the ſymptoms. During winter they returned; and at the latter end of January the patient was greatly emaciated, had a fever-fit every evening, followed by night-ſweats; his cough was ſevere, expectoration purulent, and he complained of pain under his left ſhoulder-blade, with ſome wheezing. Pulſe 100—125.

I ordered half an ounce of decoction of digitalis four times in 24 hours, which in three days brought on bilious vomiting with exceſſive languor and greatly interrupted pulſe, at about 80 in a minute, except during the evening exacerbation, when it roſe to 95. After the ſickneſs went off, the decoction was given in ſmaller doſes, with intention to keep down the pulſe without nauſea or languor. Theſe, however, were brought on by a doſe, which did not depreſs the pulſe in the morning below 80; and it was at this rate greatly interrupted, and unequal in force. In the courſe of three weeks, the exacerbation, cough, and pain, continued much the ſame. There were no night-ſweats. The legs had not begun to ſwell, nor did the patient complain of diarrhœa.

The tincture was now ſubſtituted in the doſe of 15 drops twice a day, gradually augmented to 35; but this doſe produced vomiting. I attempted to introduce more of the digitalis by dividing it into ſmaller doſes, more frequently repeated. The patient, who was extremely intent upon giving the remedy a proper trial, carried the quantity of drops by gradual augmentation, up to 100 in the day: but bilious vomiting followed.

The pulſe in the evening was never lower than 90, though it was near 60 before riſing. The pain and uneaſineſs of the left ſide have rather increaſed; the wheezing has become very troubleſome; the exacerbation is not leſs;

the ſtrength and fleſh have declined: much torpor and unrefreſhing ſleep, without any opium, through the whole courſe. The appetite has been generally worſe; though the digitalis was ſometimes taken in infuſion of quaſſia, and ſometimes in nitrous acid diluted. It was moſt agreeable in tea.

At the ſuggeſtion of the patient, who for many months had accurately attended to his own pulſe, I made the following obſervation four different days; and he told me I might have made it almoſt every day. The pulſe being rather above 80, regular and weak, when he was ſitting, he lay along the ſopha for one, two, or three minutes, when his pulſe became irregular, ſtronger, and ſlower, falling ſometimes 20 ſtrokes in a minute; and upon raiſing himſelf ſlowly, it immediately returned to its former ſtandard.

What is the cauſe of the difference between my ſucceſs and that of my two able correſpondents? From Dr. Fowler's ſtatement it is clear that it does not lie altogether in the period of the diſeaſe. I have ſuſpected that at firſt when

I uſed the decoction, I preſcribed it too largely; yet when I again refer to Dr. Fowler's reports, I am obliged to reject this ſuppoſition. In the patient whoſe expectoration of mucus was ſtopped by an attack of ſevere ſickneſs (a circumſtance which inſpired me with hopes of eventual ſucceſs) the purulent part of the expectoration was diminiſhed at each of the ſubſequent ſickneſſes: and the effect of ſea-ſickneſs upon many phthiſical patients, forbids me to conclude that the medicine failed from the too great ſeverity of its action.

In late publications, a number of facts have been ſtated reſpecting the comparative operation of diuretic remedies. But as we before knew in groſs the action of theſe remedies, and as there is little or nothing in the relation of the caſes to enable us to decide *why* one ſucceeded when the other failed, it does not appear that our practice is rendered much more certain, or our theory more luminous: our knowledge is ſtill in the groſs as it was before. If the chances in phyſic were to be calculated like caſts of the dice, theſe might be very good data. But human beings (in this reſpect unlike dice) have *interior* differences; and in his diſcernment here, are beſt ſeen the ſtrength of the phyſician's philoſophy, and the utility of his writings.

The preceding facts on both ſides being allowed, what difference of reſult exiſts, muſt ariſe from the difference in the patients' conſtitutions, as affected by the diſeaſe, or anterior to its origin. Having rejected the firſt circumſtance as inapplicable to *all* the facts, I ſhall endeavour to avail myſelf of the ſecond for an explanation. I am unwilling to impute my failure to the uſe of opium, of which I tried in vain to get rid.

My patients were all ſlender, delicate, puny, or feeble. They had all been delicately educated, in which reſpect they muſt have differed widely from hoſpital patients. Now may I not aſſume (what in an *Eſſay on conſumption* I ſhall immediately endeavour to prove by a copious induction of facts) that almoſt all the peculiarities in the mode of life among the more opulent claſſes, tend to leſſen the contractile power of the muſcular fibre; and certainly not leſs than the reſt, the contractility of the lymphatic veſſels? If ſo, I believe it will be eaſily allowed that they will be leſs within the power of the aſſociation of motions, as well as of direct ſtimuli. I have long believed it to be a principle of the animal œconomy, that in weak habits the ordinary or natural connection between different ſets of moving fibres, (or the irritative aſſociations) are alſo weak. In ſuch caſes the ſtomach may be affected; the heart may be affected; yet the lymphatics of the lungs, which in leſs feeble or more irritable habits are excited into action, ſhall continue inert. The facts which I have related concerning the two laſt patients prove that the ſtomach was affected in all degrees, conſiſtent with ſafety to life, by the digitalis; and yet that no adequate excitement of the abſorbents was produced. I do not offer this opinion for acceptance, but merely to be compared with future facts. When Dr. Fowler favours us with thoſe comments he gives reaſon to expect, perhaps he will help us to a better explanation.

Few people are better qualified for illuſtrating the actions of the animal œconomy.

In five caſes of imminent or incipient conſumption, the uſe of digitalis has either removed the complaint, or by producing the moſt decided good effects, affords hope of ſucceſs.

A young lady, with light eyes and hair, of very feeble conformation, narrow cheſted, with elevated ſhoulder blades, and a very quick pulſe, complained of a hard cough, which had ſucceeded to a ſhort hecking cough, of ſhooting pains in the cheſt, and more fixed pain in the left ſide, ſhortneſs of breath, chillineſs, and evening feveriſhneſs, ſucceeded by night-ſweats. She had lately begun to expectorate, but I was not permitted to ſee the expectoration. Small doſes of digitalis continued for three weeks, commonly at the rate of three grains of the powder in the 24 hours, removed all theſe ſymptoms. No ſickneſs was produced; on the contrary, the appetite was reſtored under the uſe of the medicine.

Two other caſes nearly ſimilar terminated equally favourably: only that in theſe, ſickneſs was produced ; in one by twenty-eight drops of the tincture ; in the other by thirty-five ; and it was neceſſary to keep the doſes between twenty and thirty.

The other two caſes are in progreſs, and ſhall be fully related hereafter. In one the attack was by far the moſt ſevere I have ever known. After a hard cough of two months continuance, attended by decline of fleſh and ſtrength, conſtant indiſpoſition, diſtinct evening fever-fit, with pulſe at 130, but no expectoration, a moſt violent pain ſeized the left ſide, which rendered coughing excruciatingly painful. The patient's habit, weak pulſe, and general loſs of ſtrength, appeared to me to prohibit general bleeding ; and topical was reſiſted. I truſted, therefore, to the tincture of digitalis. This in a month has removed the cough entirely ; has much reduced the evening exacerbation of fever, which in ſpite of two doſes of the tincture, raiſes the pulſe to 90, though it be 60 in the morning. The pain of the ſide continues, and mixes a good deal of anxiety with hope. It is however leſs.

The doſe was gradually raiſed from 15 to 30 drops twice a day; but 30 produced bilious vomiting. More than 20 cannot be taken without conſiderable nauſea, vertigo, or indiſtinctneſs of viſion. In all theſe caſes except the firſt, I found it impoſſible to avoid great nauſea, and to keep the pulſe below 80. It would be 50 in the morning, and near 100 in the evening. I ſuſpect that in people of feeble habit, the digitalis will loſe its effect on the pulſe ſooner than in others; and I ſuppoſe the above hypotheſis applicable to the fact.

No other medicine, except an occaſional aperient, was preſcribed. Great ſleepineſs ſeemed the gradation between the ordinary ſtate and depreſſion. The patient obſerved that it was "the moſt ſleepy thing ſhe had ever taken."

The other caſe now in progreſs is probably not a caſe of tubercular conſumption. The reſt I take to be certainly ſo; and I apprehend the great efficacy of the digitalis will be experienced in tubercular conſumption.

An immediate emulation muſt be produced by theſe reports among medical obſervers. But

I will beg leave to ſuggeſt to them, that the determination of the powers of digitalis is not the only, perhaps after what has been already done, not the greateſt, object of purſuit. We may preſume that nothing ſtands alone in nature. And a ſubſtance of ſimilar effect on the ſtomach and arterial ſyſtem may lurk among the articles of the materia medica. It may be detected in ſome of the bitters, given in greater doſes than common. It is impoſſibe here not to think of chamomile which in a certain doſe ſickens, and of horehound which is the general domeſtic remedy in hæmoptyſis and bad coughs. Theſe and other ſubſtances given upon the above plan may exert virtues ſimilar to thoſe of digitalis. Chemical analyſis may furniſh light here, on which account I am glad Mr. Davy has engaged in the analyſis of the digitalis. The diſcovery of an analogous body would add prodigiouſly to future ſucceſs in the treatment of conſumption.

ADVERTISEMENT.

The contents of this volume will ſhew that tolerably extenſive tracts, as well as ſmaller papers, ſuit the editor's deſign. Such tracts will be thus better preſerved, and perhaps be more widely circulated, than when publiſhed alone. If the collection goes on, I ſhall certainly not ſacrifice utility to variety. I know nothing in authorſhip more deteſtable than the practice, ſo common in periodical works, of tantalizing readers by morſelled information.

If the preſent volume appear ſomewhat later than was announced, its bulk and value will be a ſufficient excuſe for the ſhort delay. Should the obſervations of Dr. Drake and Dr. Fowler be confirmed, it will undoubtedly prove the moſt valuable medical production ever offered to the Britiſh public.

I have reaſon to hope that the firſt part of a ſecond volume may be ready by the end of the year. It will probably contain experiments on light, and on the reſpiration of gaseous oxyd of azote, by Mr. Davy—ſome matter by the editor—communications from Mr. Baynton and Mr. Creaſer—obſerv. on the climate of Madeira, by a phyſician who has had good perſonal opportunities and excellent information.

My remarks on hoſpitals, whether right or wrong, will ſcandalize ſome readers. But I am very willing to aſſign to any paper, controverting my opinions and worth publication, the ſame ſtation in the ſecond which mine occupies in the firſt volume.

The application of the profits to public purpoſes may be an inducement with ſome to contribute to the undertaking.

T. B.

In two or three weeks will be publiſhed, by the EDITOR,

An ESSAY

ON

PULMONARY CONSUMPTION,

For the uſe of families.

ERRATA.

Page	Line	
44	1	of the note, for 'a cubic' read 'an.'
47		ſtrike out out all after line 7.
49	1	for 'and' read 'though.'
91		for 'chromie' read 'chrome.'
96	1 & 5	for 'phoſnitric' read 'nitric-phos.'
110	5	of the note, for '2.25' read '1.85'
111	22	of the note, for 'ſyntheſis' read 'analyſis.'
112	6	of the note, for '2.25' read '1.85.'
143	4	for 'azote' read 'nitrogen.'
168	19	for 'azotic' read 'nitric.'
196	15	omit 'and a ſuperabundance of carbon.'
276	11	for 'circumſtances. In' read 'circumſtances, in'
321	1	dele 'of'
475	18	for '*Family diet,*' read '*Family dict.*'
499	1	for 'ſingulis' read 'ſinguli.'
507	18	for 'ſpiſſut. cicutde' read 'ſpiſſat: cicutæ.'
508	20	we ſhould probably read 'one of thoſe caſes of conſumption, which &c.'

Mere literal errors are left to the reader's correction.